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ON

SCREW PROPULSION.

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ON THE COMPARATIVE EFFICIENCY OF SINGLE AND TWIN-SCREW PROPELLERS IN DEEP-DRAUGHT SHIPS.

By W. H. WHITE, Esq., *Assistant Constructor of the Navy, Member of Council.*

[Read at the nineteenth session of the Institution of Naval Architects, April 11, 1878; the Right Hon. Lord HAMPTON, G. C. B., D. C. L., president, in the chair.]

The main purpose of this paper is to place on record the facts respecting the steam trials of deep-draught twin-screw ships of the Royal Navy, and to compare their performances with those of similar ships propelled by single screws. From this comparison it may be possible to deduce some measure of the relative efficiency, as propellers, of single and twin screws; and the discussion will probably have a practical interest for ship-builders and ship-owners generally.

The opinion appears to be generally entertained that in deep-draught ships it is preferable to employ a single screw, because any assigned speed can be attained with a smaller engine-power than would be required with twin screws. This opinion is based rather upon *à priori* reasoning than on actual experience; very few trials having been made with twin screws in deep-draught merchant ships. Moreover, recent experience with ships of war points in the contrary direction; and, after a careful analysis of all the facts which I have been able to collect, my clear conviction is that in vessels of deep draught and large engine-power twin screws have hitherto proved to be more efficient propellers than single screws.

Before proceeding to give the principal *data* upon which this statement is based, I would remark that although trials on the measured mile may not compare, in scientific accuracy, with experiments like those conducted by Mr. Froude, yet the broad deductions from a large number of such trials may be trusted. Recorded performances on the measured mile include in one result the net resistance of the ship, the augmentation of the resistance due to the action of the propellers, the efficiency of the engines, and that of the propellers; errors of observation, the influence of tides or currents, and many other circumstances, must also be guarded against. It is unquestionable that a separation of these items in the performance, and an analysis of each is the process likely to yield the greatest benefits, and to secure a just appreciation of the real causes of good or bad performance. It is not easy, however, to arrange experiments which shall correctly indicate the value and tendency of each of these constituent parts, as well as eliminate disturbing influences; and for our present purpose it must suffice to have recourse to the results of carefully conducted measured-mile trials, like those to which I now invite attention.

Table I contains the particulars of the steam trials of two single-screw ships and three twin-screw ships, all of which are of equal length. The single-screw ships are sister ships, so also are the other three vessels; and there is a great similarity in form between the two types. Figs. 1 and 2 (Plate I) illustrate the differences so well that only a few words are needed in further explanation. The *Swiftsure* and *Triumph* have their iron hulls sheathed over with wood, and are copper-bottomed; the *Iron Duke* and her sisters have iron bottoms. The *Swiftsure* is of deeper draught, to admit of the single screw, and Fig. 1 shows that her midship section differs from that of the *Iron Duke* chiefly in having a greater rise of floor, and in being wood-sheathed. Fig. 2 shows the load water-lines of the two types, and also water-lines 10 feet below the load lines; from these it will be seen that the *Swiftsure* type is somewhat fuller than the *Iron Duke*, especially in the fore-body. This greater fullness no doubt causes some increase in resistance; but against it may be set the fact that the two single-screw ships have the advantage of coppered bottoms, whereas the twin-screw ships have iron bottoms (with a probably greater co-efficient of friction) and have also the "drag" of the shaft tubes with their supporting struts. On the whole, therefore, it appears fair to suppose that when the two types are driven at equal speeds they will experience resistances approximately proportioned to the *area of their wetted surfaces.* This assumes that their forms are so similar that their *total resistances* for any assigned speed will bear the same ratio to their *frictional resistances*, the possible difference in the co-efficients of friction for copper and painted iron being neglected.*

All these ships attained speeds approaching 14 knots, except the *Vanguard*, which nearly reached 15 knots on her first trial. In order to make the comparison more exact, I have estimated the horse-power required to drive all the ships 14 knots, assuming that the resistance varied as the *cube* of the speed within the limits of calculation. This is rather below the true law of variation, and somewhat favors the single-screw ships; but accepting the results, the relative economy of the single and twin screws will appear from the following figures:

	Mean value.
For single-screw ships (indicated horse-power, per square foot of wetted surface)	.249
For twin-screw ships (indicated horse-power per square foot of wetted surface)	.226

Being a clear gain of 11 per cent. in favor of the twin-screw ships.

It may be objected that the table discloses very marked differences between the performances of the sister ships *Swiftsure* and *Triumph*, or between those of the *Vanguard* and *Iron Duke*, or even between the load and light trials of the *Vanguard*. There is doubtless force in this objection, which, in reality, only amounts to a confirmation of the views expressed above respecting the results of measured-mile trials. But if the best performance of the single-screw ships is compared with the best for the twin-screw ships, the *Vanguard* (light) gains 19 per cent. upon the *Swiftsure;* or if the *Triumph* is compared with the *Iron Duke* there is a gain of 8 per cent. in favor of the least successful twin-screw ship. In this last comparison both ships have Griffith's screws; but the *Iron Duke* would certainly have done better had the pitch of her screw been increased, its revolutions diminished, and the waste work on frictional and other resistances reduced. Hereafter I shall give facts supporting this conclusion. It is worth adding that the performance of the *Iron Duke* is almost identical with that of the *Swiftsure*, in spite of her much greater speed of piston and propellers.

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* A fourth ship of the *Invincible* class, the *Audacious*, is omitted from Table I. Her bottom is zinc-sheathed, and this sheathing was not painted over before the steam trials; consequently its co-efficient of friction was very large as compared with that for the sister ships, and her performance cannot be compared with theirs.

In completion of this comparison, it may be stated that all these ships have engines of the same type—simple engines with horizontal cylinders; the average weight of machinery, boilers, &c., for the single-screw engines was about 2.9 cwts. per indicated horse-power; that for the twin-screw engines about 3 cwts.

Table II comprehends the performances of a number of first-class masted iron-clads, and although there are much greater dissimilarities in form, size, ratio of length to breadth, and types of engines than occur in Table I, it will be possible to obtain some kind of comparison between the single and twin-screw ships.* The figures in the table expressing the ratio, curved surface of bottom $\div$ (displacement) $^{\frac{2}{3}}$, show that there is a considerable resemblance between the forms of the different vessels; but it is to be remarked that the differences of form are, on the whole, decidedly adverse to the twin-screw ships. All the single-screw ships, and the *Captain*, were fitted with simple surface-condensing engines; the *Alexandra* and *Temeraire* have compound engines. All the ships have Griffiths' two-bladed screws, except the *Alexandra*, which has a Mangin screw. These differences between the engines and propellers, as well as the forms and proportions of the ships, of course affect the performances; but they can scarcely be considered to account wholly for the very different expenditure of power in the single and twin-screw ships.

To facilitate comparison all the ships have been assumed to be steaming at a uniform speed of 14.6 knots; the horse-power corresponding to that speed has been estimated from the measured-mile *data* on the hypothesis—known to be a fair assumption—that the resistance varies as the *cube* of the speed, within the limits of speed considered. Further, I have stated the ratio of these horse-powers to the total wetted surfaces—including bottoms, keel, bilge-keels, shaft-tubes, struts, &c.; and on the whole this is the fairest measure of the expenditure of power. But it must not be overlooked that this measure tells against the twin-screw ships; for the added resistance due to shaft tubes and struts is undoubtedly greater than the mere frictional resistances of the surfaces of these adjuncts; and further, in the cases of the *Alexandra* and *Temeraire*, which are fuller formed ships than any of the others, the "wave-making" element of the resistance will, in consequence, be proportionably greater than it is in the finer ships. When the ratio $\frac{\text{Indicated horse-power}}{\text{Wetted surface}}$ is used, as above explained, it is really assumed that the *total resistances* of all the ships compared vary with the *frictional resistances*, which is not strictly true, and which tells particularly against the *Alexandra* and *Temeraire*. Comparing the *Alexandra* and *Temeraire* with the *Hercules*, *Sultan*, and *Monarch*, the differences of form are indicated by Figs. 3 and 4 (Plate I). Fig. 3 shows the load-line sections. Fig. 4 the forms of the water-lines at mid-draught, and, on inspection, the preceding statements as to the greater fullness of the twin-screw ships will be found to be amply justified.

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* For the facts respecting the *Independencia* I am indebted to the kindness of Mr. E. J. Reed, C. B., M. P., that vessel having been tried before her purchase by the admiralty. She is now named the *Neptune*.

The *Alexandra*, *Hercules*, and *Sultan*, are of the same length, but the first-named ship is 4 feet 8 inches broader than the other two. Yet she gains 13½ per cent. upon the *Hercules*, and more than 30 per cent. upon the *Sultan*, in the expenditure of power, measured as described above. The *Temeraire* is 40 feet shorter and 3 feet broader than the *Hercules* or *Sultan*, yet she gains nearly 8 per cent. upon the *Hercules* in her expenditure of power, and 26 per cent. upon the *Sultan*. The *Monarch* is a long, fine ship as compared with the *Alexandra*, yet the latter requires rather less expenditure of power in proportion to her wetted surface; and even the *Temeraire*, 45 feet shorter and 4½ feet broader than the *Monarch*, is only 4 per cent. in excess of that ship in her expenditure of power. The *Temeraire*, it must be noted also has her bottom zinc-sheathed. Whatever may be the relative efficiencies of the simple and compound engines, it is obvious that this remarkable economy of power in the recent ships must be largely the result of the use of twin screws.

In comparing the *Temeraire* and *Independencia*, a few preliminary observations must be made. The *Independencia* is a somewhat finer ship, and narrower in proportion to her length. She has a coppered bottom, whereas the *Temeraire* has a zinced bottom, painted over before the trials, but probably inferior in smoothness to the copper. In the *Independencia* there are no bilge-keels, but in the *Temeraire* deep bilge-keels, as well as the shaft-tubes and their supporting struts add to the resis. tance, and probably add considerably to it. Yet the *Temeraire* gains no less than 26 per cent. upon the *Independencia* in her expenditure of power. The performance of the latter ship will be seen to be almost identical with that of the *Sultan*.

Further, compare the *Captain* and *Monarch*: the former has somewhat the advantage in fineness of form, but against this advantage must be set the drawbacks of the shaft tubes and struts. In this case the engines and screws of both ships are of similar type; the gain in expenditure of power is about 5 per cent. in favor of the *Captain*, even when compared with this exceptionally economical single-screw ship.

The performance of the *Bellerophon*, considering her relatively small size, is very good indeed, but not equal to that of the shorter and bluffer *Temeraire.* The *Bellerophon* gains relatively to the *Hercules* and *Sultan*, not merely because she is rather more finely formed, but because she has a screw of equal diameter, although her engines develop about 25 per cent. less power. This fact suggests the further consideration that since the extreme draught of war ships does not exceed 27 or 27½ feet, there must be considerable difficulties in the way of securing economical performance with single screws in cases where the indicated horse-power has to reach 8,000 horse-power or upwards, unless the general practice in proportioning the diameters of screws to the indicated horse-power is entirely departed from. Increased speeds of screws and of pistons are, of course, possible, but they have their accompanying difficulties and drawbacks; and it seems probable that the marine engineer will have to exercise his skill in devising new types of engines, if the single screw is to hold its place in competition with twin screws. The latter plan enables an ample diameter and disc area of screw propellers to be obtained for the highest amount of engine power likely to be employed, without exceeding the limits of draught named above, and without any extraordinary speed of propellers and pistons.

In concluding these remarks on Table II, it may be worth mention that if the various differences specified above are disregarded, and the *mean values* obtained for the ratios of indicated horse-powers to the wetted surfaces, they stand as follows:

	I. H P.
For the single-screw ships per square foot of wetted surface	.31
For the twin-screw ships per square foot of wetted surface	.26

Being a clear gain of 18 per cent. in favor of the twin screws. This is, of course, only a rough mode of comparison.

The excellent performances of the *Devastation* and *Thunderer* on the measured mile furnish still further evidence of the efficiency of twin screws. The *Thunderer* was tried under circumstances of wind and sea which were less favorable than those under which the *Devastation* was tried, and this fact probably accounts for the difference in their performance. Taking the *Devastation's* measured-mile trial it appears that the ratio of the "effective" horse-power, to the "indicated" horse-power was about 42.5 to 100; and it will be remembered that Mr. Froude fixes about 37 to 100 as a good corresponding ratio for single-screw ships. This indicates about 15 per cent. of gain for the *Devastation* as compared with the average performance of single-screw ships. It is obvious that the absence of rigging and sails in the *Devastation* helps somewhat towards this economy of power; but the twin screws probably help a great deal more. This vessel (285 feet long, and 62¼ feet broad) was driven 13.8 knots with an expenditure of only .235 horse-power per square foot of wetted surface; and with an expenditure of .25 horse-power per square foot she could have been driven 14 knots—results which are certainly remarkable when the full form of the ship is considered. The *Dreadnought* also on her light-draught trial did exceedingly well. She is 35 feet longer and 14 inches broader than the *Devastation*, but has virtually the same entrance and run. With an expenditure of .27 horse-power per square foot of wetted surface she attained a speed of about 14½ knots—a result comparing very favorably with those for the masted ships in Table II. But there is every reason to anticipate that even a better result will be obtained when the *Dreadnought* has her deep-draught trial, the additional immersion of the screws having, as a rule, added to their efficiency in twin-screw ships. The *Alexandra*, for instance, at her first trial on the Maplin, at a mean draught of 24 feet 1 inch, with a displacement of 8,442 tons and a wetted surface of 28,200 square feet, required 8,489 horse-power to drive her at a speed of 15 knots—.301 horse-power per square foot of wetted surface. At her load-draught trial (see Table II) for the same speed the expenditure of power was only .29 horse-power per square foot of wetted surface. The corresponding trials for the *Temeraire* showed even a more striking difference. At the light draught the expenditure of power was .295 horse-power per square foot of wetted surface for a speed of 14.56 knots; whereas at the load draught the corresponding expenditure was .279 horse-power for a speed of 14.65 knots, the screws being immersed 14 inches deeper than on the first trial. It must be added that part of this economy of power in the *Temeraire* was undoubtedly due to an increase in the pitch of the screw and to a consequent diminution in the number of revolutions (from 79 to 74) which carried with it a diminution in the waste work of screw frictional and edge resistances, engine friction, &c. The *Vanguard* furnishes an apparent exception to the rule that deeper immersion of the twin screws adds to their efficiency (see Table I). It is only proper to note, however, that the peculiar form of the stern on the *Vanguard* may have led to a sensible increase in the resistance when it was immersed 18 inches deeper than at the earlier trials.

From the foregoing summary of experience with Her Majesty's ships it may safely be affirmed that twin screws have proved more efficient than single screws, as regards economy of power. I do not propose in this paper to examine into the possible causes of this superiority, further than they suggest themselves in the tables and the remarks made thereon. The matter is, however, one well deserving inquiry, and will I doubt not receive the benefit of Mr. Froude's experimental investigation. I cannot refrain from noting, however, that the additional resistance incurred in carrying out the shaft-tubes and fixing their struts, is evidently more than compensated for by the efficient action of the screws placed in a good run of water and well clear of the hull. What may be the comparative "augment" of resistance for twin and single screws only experiment can determine.

One other case that has come to my knowledge bearing on the subject, and confirming the previous examples, may here be noted. The *Avni Illah* and *Muini Zaffer* are two Turkish iron-clads, the first built by the Thames Iron Works, and the second by Messrs. Samuda, both having been engined by Messrs. Ravenhill. They are practically sister ships, I believe, but the former has a single screw, while the latter has twin screws. The *Muini Zaffer* is said to have exceeded 13 knots with 2,555 horse-power and a displacement of nearly 2,100 tons, with force of wind 7; whereas the single screw *Avni Illah* only reached 12 knots with 2,450 horse-power and a displacement about 100 tons greater than that of her sister ship, the force of wind being 5. If the two vessels were made equal in displacement, the *Avni Illah* would have probably required 3,200 horse-power to reach the speed of 13 knots, which the *Muini Zaffer* attained with about 2,500 horse-power—a gain of 28 per cent. for the twin screws. I am not able to complete the comparison for these ships beyond this point, but it is an interesting case, and deserves to be put on record in detail.

Next I have to notice the comparative trials of two ships, which appear, at the first glance, to contradict our experience in the Royal Navy. Table III gives the facts, for which I am indebted to Mr. Walter Brock, of Denny Brothers, Dumbarton. It is understood that the two ships were practically sister ships; they were, I believe, built and engined by Messrs. Napier. The broad facts of the case are as follows: The single screw ship was driven 14.64 knots, with 3,107 horse-power, whereas the twin-screw ship only reached 13.83 knots with 3,793 horse-power. This is certainly a very inferior performance for the twin-screw ship. When the table is carefully examined, however, a very curious anomaly becomes apparent:

(1.) The twin screws have an aggregate disc-area more than 40 per cent. in excess of the corresponding area for the single screw; therefore one would expect the twin screws to have the *less* percentage of slip.

(2.) Instead of having less slip than the single screw the twin screws have 13.8 per cent., as against 5.2 per cent.

Hence it may be inferred that the twin screws were not well adapted for their work; and their failure ought not to be construed into a condemnation of the use of two screws, but rather into an argument against the form and arrangement of the twin screws in this particular vessel. Moreover, it will be noticed that the types of engines in the two ships differed widely, and tended to make the comparative performances of the ships less trustworthy as a measure of the relative efficiency of single or twin screws. This feature was, however, probably less influential than the inefficiency of the twin screws noted above.

It is not at all improbable that the performance of the *Washington* might have been greatly improved had some other kind of screw been tried. Several cases might be adduced in support of this opinion. The *Invincible*, for example, was first tried with two Mangin screws 16 feet 2 in diameter, and only 17 feet 2 pitch, and attained a speed of 13¾ knots, with 5,178 horse-power. Her performance was so much inferior to that of the *Vanguard*, as to lead to a careful comparison of the forms and positions of the screws in the two vessels, the engines being so similar as to leave little room for supposing any important difference to arise in connection therewith. After being fitted with other Mangin screws of the same form as those of the *Vanguard*, and placed about a foot further aft than the first screws, the *Invincible* performed as shown in Table I. Her gain in speed, due entirely to the change in screws, was estimated at .6 of a knot; that is to say, with 5,178 horse-power she would have probably reached 14.4 knots, and with 4,832 horse-power she actually realized 14.093 knots. The increased pitch led to a decrease in the number of revolutions from 81 to 71, and this resulted in a very important diminution in the waste work of engine friction, &c., as well as of frictional and edge resistance to the motion of the screws. With Mangin screws having large blade-areas this is obviously a matter requiring careful consideration and only to be settled by actual trial. Reverting to the Table III, it will be seen that the *Washington* has an area of surface of her blades of 185 square feet, as against 158 square feet for the *St. Laurent;* and this greater blade-area was making 78 revolutions per minute as against 69 for the single screw. Here we have a fruitful source of " waste work " for the twin screws as compared with the single screw.

The determination of the best forms and positions for twin screws is a matter which will be largely settled by further experience and experiment. It is sometimes forgotten that whereas the single screw has had the great advantage of thirty or forty years' employment and improvement, the use of twin screws is of much more recent date, and their employment in deep-draught ships is only now becoming common. It is scarcely to be anticipated, therefore, that in all cases an unmixed success will be obtained in the first attempt to use twin screws under new conditions. This was not so in the *Invincible*, but the cause of inferior performance was discovered and remedied. Had the *Washington* been similarly experimented with her performance would doubtless have been much improved. The recent trials of Her Majesty's ship *Iris* furnish a still more remarkable illustration of the foregoing statements; and brief reference must be made to them, although it should be understood that the experiments with her propellers are not yet concluded.

The *Iris* is a dispatch vessel, 300 feet in length, 46 feet broad, and 22 feet extreme draught; her load displacement is about 3,700 tons, and the estimated speed 17 to17½ knots. For a vessel of her comparatively small size and moderate proportions this is obviously an extraordinarily high speed, and it was estimated that the engines must develop at least 7,000 horse-power in order that the speed should be reached. To secure efficient propulsion, without having recourse to exceptional speeds of pistons and propeller, it was therefore necessary to use two screws, the draught not admitting of a sufficiently large single screw, if the ordinary practice in proportioning diameter to engine power was to be followed approximately; and this arrangement was essential even if twin screws had been considered less efficient propellers. When first tried the *Iris* had two four-bladed screws, each 18½ feet in diameter; the joint disk area of the screws was therefore considerably greater in proportion to the engine power than is common in single-screw ships, and the surface of the screw blades was exceptionally large. The results of the trials with these four-bladed screws are illustrated (so far as speed and power are concerned) by the curve A, Fig. 5; and they were certainly disappointing. The vessel attained a speed of 16.6 knots with 7,500 indicated horse-power and 91 revolutions per minute. It was then arranged, for experimental purposes only, to remove two out of the four blades on each screw, and to make a series of progressive trials, in order to see what improvement would result from a reduction in the blade area. The curve B, Fig. 5, exhibits the results of these trials, and presents a most remarkable contrast to the curve A. The highest power developed on this trial was 4,369 horse-power, the corresponding speed 15¾ knots, and the revolutions 89. With the four-bladed screws 15⅛ knots had required 5,250 horse-power, and 4,369 horse-power would have sufficed for 14⅓ knots only. It was not desirable to press this extemporized two-bladed screw by running at any higher speeds, nor was its form or pitch adapted for use as a two-bladed screw. Enough had been done to show that the performance of the ship could be greatly improved by a modification in her screws, and steps are now being taken to supply her with suitable screws. What diameter and form of screw will ultimately be adopted is a question to be settled by further experiments.

In some of the published accounts of these trials with the *Iris* an opinion has been expressed that better results would have been attained with a single screw. This opinion is, to say the least, speculative; for there has never yet been a trial with a single-screw ship, of the same moderate draught, in which engines of equal power have been employed. It need not be said that moderate draught is of the greatest importance in a dispatch vessel; and, accepting this as a fixed condition, it follows that a single screw could not exceed 20 feet in diameter. Its disk area would be 314 square feet, and upon this 7,000 to 7,500 horse-power is to be applied—a ratio of power to disk area which is, I need scarcely say, exceptionally great. Even the torpedo vessel *Lightning* has a disk area of 23¾ square feet for her relatively enormous power of 400 horse-power; and in the *Inconstant* and *Shah* screws of 23 feet diameter are employed for from 7,000 to 7,500 horse-power developed. It is clear, therefore, that if a single screw had been tried in the *Iris* it must have been associated with conditions in the design of the machinery, in the speeds of piston, screw, &c., or in the ratio of pitch to diameter, which have never yet been tried on such a large scale; in fact the trial must have been largely experimental. And, even if success had been achieved with a single screw, the vessel would necessarily have been deprived of many advantages she now possesses in virtue of her twin screws.

The exceptionally fine form of the *Iris* and the large diameter of her screws rendered it necessary to carry the shafts outboard for the unusual distance of 50 feet. To support the tubes through which the shafts are carried two wrought-iron struts are fitted on each side, and these necessary adjuncts to the hull proper add very considerably to the resistance. This fact, of course, tells against the twin screws, and some fittings of the kind are unavoidable, although further experience may lead to arrangements of struts, &c., which will produce less resistance than those now in use. As we have no trials of single-screw ships of similar form and size with the *Iris*, an exact comparison of her performances with those of such ships cannot be made. In all probability, however, if previous practice were conformed to in engines and screw, about 16 knots is the limit of speed at which ships of the same draught with the *Iris* and having a single screw could be economically driven. But, if any such comparisons should be attempted at any time, it must be borne in mind that the *Iris*, when steaming 16 knots, is developing only about two-thirds the full power of her engines, and therefore experiencing an unusually large percentage of waste work on engine friction, &c.

Although no comparison is possible with another single-screw ship, a comparison can be made between the trials of the full-sized ship and those made by Mr. Froude with her model. It would be out of place to discuss the subject here, but I may be permitted to make one statement which seems to have considerable practical importance: The twin screws, placed well clear of the hull, are exceptionally efficient as propellers, and make the ratio of "effective" to indicated horse power much higher than is usual in single-screw ships. Mr. Froude fixes this ratio at from 37 : 100 to 40 : 100 in good examples of single-screw ships. Taking the 15¾ knots speed for the *Iris*, it appears that if the "effective" horse power is estimated from her total resistance—including shaft, tubes, struts, &c.—its ratio to the indicated horse-power rises to 44 : 100 Even if the "effective" horse-power be estimated from *the resistance of the hull only*, stripped of these adjuncts, the ratio to the indicated horse-power is above 38 : 100. In other words, the superior propelling efficiency of the twin screws makes amends for the increase of resistance due to shaft, tubes, struts, &c., and for the exceptionally large proportion of waste work on the engine friction due to the engines working at less than full power, even at a speed for which a single screw could work economically under the conditions of present practice, on the draught of water of the *Iris*. If the twin screws thus hold their own at speeds up to 16 knots, they will probably do much better than a single screw at the higher speeds for which the *Iris* is designed, when 50 per cent. more power is to be put upon the screws than is required for 16 knots. The further trials of the ship will be watched with interest by all connected with the construction of steamships; but so far as the present trials have gone, they decidedly furnish an argument in favor of the use of twin screws.

Apart from the question of their efficiency as propellers twin screws are the source of some advantages, which should certainly be taken into account in comparing them with single screws. These are—

(1.) Greater security against total disablement of the propelling apparatus.

(2.) Greater handiness, and the maintenance of maneuvering power in case of serious damage to the rudder or steering gear.*

(3.) Greater facilities for the water-tight subdivision of the engine-room by means of middle-line bulkheads.

The first-named advantage is that which will probably have most weight with ship-owners. It is unnecessary for me to cite cases illustrating the serious character of a "break-down" in the machinery of an ocean-going steamer with a single screw and a small sail spread. But it is obvious that the danger increases with the length of the voyage; and that an accident which would be serious to an Atlantic mail steamer might be much more serious if it happened to a vessel engaged in the Australian service. The gain in safety obtained with twin screws would surely be worth having, even if it entailed a somewhat greater expenditure of engine power and loss of carrying power; but I have shown that no such price has to be paid. On the contrary, there is reason to expect some economy of power with twin screws. Their use in merchant ships would, doubtless, be attended with some difficulties and disadvantages; and I have endeavored to ascertain what are the chief objections, unconnected with propulsion, that are raised against the employment of twin screws. These seem to be as follows:

(1.) Greater liability of damage to the screws when ships are going into or out of docks, coming alongside wharves, or taking the ground.

(2.) Reduced cargo space, in consequence of a larger engine-room and two shaft passages being required.

(3.) Necessity for a larger and more expensive engine-room staff, in consequence of the machinery being duplicated.

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* With *ordinary* rudders the use of twin screws does not appear to interfere with the efficient action of the rudder when both screws are working ahead, as compared with that in single-screw ships; but experience has shown that *balanced* rudders are not desirable features in twin-screw ships. With steam or mechanical steering gear the use of balanced rudders is, on other grounds, not preferable; so that this feature in the use of twin screws is of comparatively small importance.

(4.) Increase in the weight of the machinery, in proportion to the horse-power developed, as compared with single-screw engines.

Respecting these objections only a few comments will be needed, since they are all matters of fact and experience. There is obviously considerable force in the first objection. More care is requisite to prevent damage to twin screws than is usual with single screws. It is worth notice, however, that several years' experience with Her Majesty's ships have shown that without any extraordinary precautions twin screws can be kept efficient. Merchant ships, no doubt, have to sustain rougher usage than ships of the Royal Navy, and their smaller beam is less favorable to the provision of shelter for twin screws. On the other hand, the merchant ships would require screws of smaller diameter in proportion to their load draught than would be necessary in ships of war, because of the comparatively small engine-power needed to drive the merchant ships. The smaller diameter of the screws favors their continued immersion as the ships lighten, and also leads to a decrease in the length of the shaft-tubes outside the hull. I find on examination that even the fullest powered mail steamers would not require a length of shaft-tubes more than one-half as long as those of the *Iris*, and a single strut on each side would suffice to support these tubes. As compared with the *Iris*, therefore, the increase of resistance due to the struts and tubes would be very considerably less in any merchant steamer, and the screws would be much more sheltered. Even if this were not so I would submit that it might be no difficult matter to arrange a protective "guard" or casing for the twin screws of merchant ships, if such a feature should prove necessary. And, in any case, it seems unwise to wholly put aside the proved advantages of twin screws because of this difficulty, which must certainly be surmountable.

As to the effect of twin-screw engines upon the cargo space I can give no opinion; but I have been assured by competent authorities that in most trades the substitution of two shaft passages for one would produce no serious loss of carrying capacity; and I may add that in war ships this holds good. With twin screws good "square" stowage is obtained, easily accessible from hatchways at the middle line. With a single-shaft passage, only the wing spaces are available, and the passage being at the middle line renders the stowage of the after-hold less easy. Further, it does not seem probable that with compound engines of the inverted cylinder type the use of twin-screw engines need entail any increase in the space assigned to the machinery. If the engines are placed abreast of one another, it seems likely that the length of the engine room may be somewhat less than with single-screw engines of equal power, and the additional athwartship space required might be compensated for by the reduction in length. In the ships of the Royal Navy the engine-rooms in the twin-screw ships are, on the whole, shorter than those for single-screw ships with engines of similar type and equal power. For example, the engines of the *Swiftsure* occupy 30 feet of length, whereas those of the twin-screw ship *Vanguard* occupy 28 feet; and the *Bellerophon*, with 6,500 indicated horse-power, has an engine-room 40 feet long, as against 36 feet for the *Captain*, with 6,000 horse-power. In recent ships the adoption of the inverted cylinder type of engine has tended to further economize space, transversely as well as longitudinally.

Respecting the third objection, I need not say anything; but a few words are necessary as to the fourth. Taking a considerable number of Her Majesty's ships, engined by different firms, I find that for simple engines with horizontal cylinders and surface condensers, the weight per indicated horse-power of the single-screw engines is about 7 per cent. less than that for twin-screw engines. For compound engines, however, the average weights per indicated horse-power are as nearly as possible the same for single and twin-screw ships. These statements must, of course, be considered in connection with the foregoing remarks on the propelling efficiency of single and twin screws. Although engines in the merchant service are heavier in proportion to their power than those fitted in Her Majesty's ships, this comparison of the weights of single and twin screw engines will probably hold good; and it certainly contradicts the general assumption that twin-screw engines are necessarily heavier in proportion to their power.

In conclusion, I would add a few general remarks on the employment of twin screws in ocean-going steamers of the mercantile marine. These vessels are very long and narrow, even when contrasted with the *Iris;* and I am not forgetful of the fact that their great lengths and fine forms so considerably diminish their resistance that single screws, of ample diameter in proportion to their engine power, can be employed efficiently on the working draughts of water. If twin screws were used, however, their average performance would probably be better on a long ocean voyage, involving a considerable variation in the draught, than that of single screws; because the twin screws would be of smaller diameter and more deeply immersed. For the same reason *pitching* would be likely to produce less effect upon the performance of the twin screws than on that of single screws. Rolling motions could scarcely produce any sensible effect on well-immersed twin screws, although their position at some distance out from the middle line places them at some disadvantage, as to angular motion, compared with single screws. On the whole, therefore, it may be concluded, from the above-stated results of experience as well as from general considerations, that the twin screws would have a decided advantage regarded as propellers only.

In order to carry the twin screws well aft, external shaft-tubes and supporting struts are needed. I have already stated that in no existing merchant steamer need these external tubes be more than half as long as those of the *Iris;* and that the additional resistance or "drag" of the tubes and struts must be small as compared with the corresponding feature in the *Iris.* The analysis of the performance of that ship shows that the twin screws more than compensated for this "drag" by their superior efficiency; consequently there need be no apprehension that in mail steamers the corresponding "drag" would cause a loss of speed. The objections that may be raised to the exposure to injury of the shaft-tubes, struts, and screws have already been considered. So, also, has the practical guarantee against disablement afforded by the use of twin screws. I may leave the judgment as to the relative importance of these two matters in the hands of ship-builders and ship-owners.

Looking to the future of steam navigation, one thing seems certain: greater speeds will be attained than are now reached. It does not seem probable that any considerable increase in fineness of form, or in the ratio of length to breadth, will be adopted in future ships for the purpose of diminishing their resistance. Any increase in the load-draughts is also clearly inadmissible. The greater engine powers which will probably be used in the swifter ships will consequently have to be applied on a limited draught of water; and hence it may be anticipated that at no very distant date the designs of swift mail steamers will be subject to conditions resembling those sketched above for the *Iris.* The extreme draught will not permit a use of the single screw with a disk area bearing anything like the ordinary ratio to the indicated horse-power; and it will become necessary either to depart from established precedents in the ratio of pitch to diameter of single screws, to adopt twin screws, or to accept greater speeds of pistons and propellers than are now common in large marine engines. At the present moment there is no great urgency in deciding between these rival methods, and I have no wish to attempt a prediction as to the practice of the future. The matter is, however, one well deserving the careful consideration of marine engineers and naval architects.

TABLE I.

	Single screws.		Twin screws.			
Name of ship	*Swiftsure.*	*Triumph.*	*Vanguard.*	*Vanguard.*	*Invincible.*	*Iron Duke.*
Length between perpendiculars	280 ft.	280 ft.	280 ft.	280 ft.	280 ft.	280 ft.
Breadth extreme	55 ft.	55 ft.	54 ft.	54 ft.	54 ft.	54 ft.
Mean draught on trial	24 ft. 9½ in.	25 ft. 7½ in.	22 ft. 6 in.	21. 0 ft.	21. 0 ft.	21. 0 ft.
Displacement in tons	6, 537	6, 840	6, 076	5, 563	5, 563	5, 563
Area of immersed midship section, square feet	1, 140	1, 186	1, 077	996	996	996
Area of wetted surface, square feet	22, 120	22, 620	21, 750	20, 850	20, 850	20, 850
Character of bottom	Coppered.	Coppered.	....	Iron, clean	painted.	....
Screw propellers:						
Description	Griffiths.	Griffiths.	Mangin.	Mangin.	Mangin.	Griffiths.
Number of blades on each	2	2	4	4	4	2
Area of surfaces of blades, square feet	137	137	304	304	304	209
Diameter	20 ft.	20 ft.	16 ft. 2 in.	16 ft 2 in.	16 ft. 2 in.	16 ft. 6 in.
Pitch	24 ft. 6 in.	24 ft. 6 in.	20 ft. 9 in.	20 ft. 9 in.	20 ft. 9⅞ in.	19. 0 ft.
Immersion of upper edge	4 ft. 1 in.	5 ft. 1 in.	4 ft. 4 in.	2 ft. 10 in.	3 ft. 5 in.	3 ft. 3 in.
Area of propeller disk, square feet	314	314	412	412	412	428
Revolutions per minute on trial	68. 4	67. 85	72	73. 7	70. 8	81. 2
Trials:						
Where tried	Plymouth.	Plymouth.	Plymouth.	Plymouth.	Plymouth.	Plymouth.
When tried	Jan. 11, 1872.	Mar. 19, 1873.	Oct. 15, 1870.	April 7, 1870.	Sep. 26, 1870.	Aug. 18, 1870.
Force of wind	2 to 3.	5	2	2 to 3.	1	1
State of sea	Moderate.	Smooth.	Smooth.	Smooth.	Smooth.	Smooth.
Indicated horse-power	4, 913	5, 114	5, 312	5, 366	4, 832	4, 789
Speed of ship, in knots	13. 755	13. 522	14. 076	14.944	14. 093	13 855
Slip of screws—per cent	16. 79	17. 57	4. 6	. 93	1. 89 (neg.)	8. 99
At 14 knots:						
Indicated horse-power	5, 275	5, 875	5, 200	4, 140	4, 710	4, 995
Indicated horse-power per square foot of wetted surface	.2385	. 2597	. 2391	. 2	. 2259	. 2396

TABLE II.

	Single screws.					Twin screws.		
Name of ship	*Bellerophon.*	*Monarch.*	*Hercules.*	*Sultan.*	*Independencia.*	*Captain.*	*Alexandra.*	*Temeraire.*
Length between perpendiculars	300 ft.	330 ft.	325 ft.	325 ft.	300 ft.	320 ft.	325 ft.	285 ft.
Breadth extreme	56. 0 ft.	57 ft. 6 in.	59 ft.	59 ft.	63 ft.	53 ft. 3 in.	63 ft. 8 in.	62 ft.
Mean draught on trial	23 ft. 11½ in.	23 ft. 8½ in.	24. 8¼ ft.	24 ft. 10¾ in.	24 ft. 8 in.	24 ft. 10 in.	26 ft. 1½ in.	27 ft.
Displacement, in tons	7, 236	8, 070	8, 676	8, 714	8, 950	7, 672	9, 432	8, 571
Area of immersed midship section, square ft.	1, 188	1, 208	1, 313	1, 320	1, 453	1, 176	1, 405	1, 455
Area of wetted surface, square feet	25, 400	27, 450	28, 000	28, 140	25, 430	26, 260	29, 620	26, 900
Area of curved surface of bottom (exclusive of bilge keel, &c.), + (displacement) ⅔.	59. 6 ft.	61. 4 ft.	60 ft.	60 ft.	58. 3 ft.	62 ft.	59. 7 ft.	57. 2 ft.
Character of bottom	...	Iron, clean painted.		...	Coppered.	Iron, clean painted.		Zinc, clean painted.
Screw propellers:								
Description	Griffiths.	Griffiths.	Griffiths.	Griffiths.	Griffiths.	Griffiths.	Mangin.	Griffiths.
Number of blades on each	2	2	2	2	2	2	4	2
Area of surfaces of blades, square feet	210	254	210	210	136	200	473	263
Diameter	23 ft. 6 in.	23 ft. 4 in.	23 ft. 6 in.	23 ft. 7 in.	22 ft. 6 in.	17 ft.	21 ft.	20 ft. ½ in.
Pitch	20 ft. 1 in.	26 ft. 4 in.	24. 0 ft.	24 ft. 1 in.	23. 0 ft.	21 ft. 8 in.	22 ft. 2¾ in.	22 ft. 6 in.
Immersion of upper edge	2 ft. 2 in.	1 ft. 4 in.	1 ft. 11½ in.	2 ft. 4 in.	1 ft. 11 in.	4 ft. 10 in.	3 ft. 3 in.	4 ft. 11 in.
Area of propeller disk, square feet	434	428	434	434	398	454	692	628
Revolutions per minute, on trial	75	63. 6	71. 5	71. 5	70. 5	74. 3	64. 2	73. 8 ft.
Trials:								
Where tried	Stokes' Bay.	Stokes' Bay.	Stokes' Bay.	Stokes' Bay.	Maplin.	Stokes' Bay.	Stokes' Bay.	Stokes' Bay.
When tried	Aug. 17, 1866.	July 17, 1869.	Jan. 1, 1869.	April 19, 1871.	Feb. 2, 1878.	Mar. 2, 1870.	Feb. 28, 1877.	Sept. 17, 1877.
Force of wind	5 to 6	1 to 2	2 to 4	4 to 5	...	3 to 4	4	4
State of sea	Little swell.	Calm.	Smooth.	Smooth.	...	Smooth.	Smooth.	Smooth.
Indicated horse-power	6, 521	7, 842	8, 529	8, 629	8, 832	5, 990	8, 615	7, 516
Speed of ship, in knots	14. 17	14. 94	14. 69	14. 13	14. 6	14. 24	15	14. 65
Slip of screws—per cent	4. 73	9. 65	13. 26	16. 85	8. 75	10. 47	6. 5 (neg.)	10. 5
At 14.6 knots:								
Indicated horse-power	7, 350	7, 151	8, 320	9. 835	8, 832	6, 619	7, 732	7, 413
Indicated per sq. ft. of wetted surface	. 2894	. 2651	. 2972	. 3495	. 3473	. 2521	. 261	. 2756

TABLE III.

	St. Laurent (single screw).	*Washington* (twin screw).
Length	....	345 ft.
Breadth	....	42. 8 ft.
Mean draught on trial	18. 11 ft.	17. 81 ft.
Displacement, in tons	4, 445	4, 474
Area of immersed midship-section, square feet	642	645. 8
Screw propellers:		
Description	Griffiths, curved forwards.	Griffiths, straight.
Number of blades on each	4	3
Area of surfaces of blades, square feet	158	185
Diameter	17. 717 ft.	15 ft.
Pitch	22. 637 ft.	20. 833 ft.
Area of propeller disk	246 sq. ft.	353 sq. ft.
Revolutions per minute, on trial	69	78
Engines:		
Description	Horizontal, geared.	Inverted, direct.
Number of cylinders	2	4
Diameter of cylinders	86. 6 in.	58 in.
Stroke	4. 265 ft.	3 ft.
Trials:		
Where tried	Cherbourg.	Cherbourg.
When tried	Sept. 30, 1866.	April 22, 1868.
Indicated horse-power	3, 107	3, 793
Speed of ship, in knots	14. 64	13. 83
Slip of screws, per cent	5. 2	13. 8

ON THE ELEMENTARY RELATION BETWEEN PITCH, SLIP, AND PROPULSIVE EFFICIENCY.

BY W. FROUDE, Esq., M. A., F. R. S., *Vice-President.*

[Read at the nineteenth session of the Institution of Naval Architects, April 11, 1878 the Right Hon. LORD HAMPTON, G. C. B., D. C. L., president, in the chair.]

Speaking generally, one may say that it is in accordance with the prevailing impressions respecting the theory of the screw propulsion, (1) that in seeking to obtain from the yielding material, water, a reaction adequate to a large propulsive force, it tends to efficiency to employ as large as possible an effective area in the propeller; and (2) that it tends to give maximum efficiency to this area, whether it be large or small, if we make it operate on the water as squarely to the line of motion as possible, and therefore with as short a pitch as possible, in order to avoid a useless expenditure of power in giving rotational motion to the water acted on. Hence the limitation in diameter of propeller has been regarded as dependent on the space practically available, and the shortness of pitch as dependent on the speed it was convenient or safe to give to the engines, and by other purely practical considerations involved in reference to both conditions.

But even on *prima facie* applicable principles, one sees that definite limiting considerations present themselves; indeed "tendencies" are generally but illusive guides, unless we can assign to each its quantitative value. For instance, admitting that slip, whether large or small, involves a proportionate waste of power, the question at once presents itself, "Would unlimited area of propeller be theoretically valuable? How much do we lose by the limitation of area imposed by mere practical convenience?" Indeed, a very exaggerated importance is sometimes attached to slip, as though to reduce slip in a given proportion were to reduce the expenditure of power in the same proportion; whereas, assuming that the slip ratio is already, say, 10 per cent., the waste of power occasioned by it is only 10 per cent. of the whole power, and to halve it would only effect a saving of 5 per cent. of the whole; and to halve it again only a saving of 2½ per cent. more; while each of these gains would require, on the simplest calculation, at least a doubled area, so that to save, in this case, 7½ per cent. of the whole power, we should, approximately, need to double the diameter of the propeller; and we have to ask ourselves what would be the other theoretically inevitable consequences of the alteration. And the same remark applies in reference to the similar apparent gain which seems to be inherent in reduction of pitch, as giving squarer sternward pressure to the blades of the propeller; for if (say) we halve the pitch, so as to give a proportionately increased squareness to the pressure, we must nearly double the number of revolutions to maintain the speed of propulsion; here also we have to ask ourselves what would be the other theoretically inevitable consequences of the alteration.

In both these cases we see at once that there must ensue a great excess of power expended in overcoming the skin friction and other elements of edgeways resistance inherent in the blades, and it becomes necessary to correlate these losses with the loss by slip. Take the case of a screw 20 feet diameter, making 80 revolutions per minute; the tips of the blades are traveling at a speed of about 50 knots; now, the resistance of a surface so short in the line of motion as a screw-blade, even when its surface is quite smooth, is as much as 1¼ pounds per foot at 10 knots, and is nearly as the square of the speed, and as each square foot of blade area involves 2 square feet of skin, the resistance of each is over 60 pounds; thus, making some allowance for thickness and bluntness, there is involved in driving it at 50 knots at least 10 indicated horse-power, and collectively the outmost foot of four such blades, each 3 feet wide, would absorb fully 120 indicated horse-power in surface friction; and though the parts of the blade nearer to the root move with proportionately less speed, and therefore with less resistance, yet on the other hand screw-blades are generally "rough from the sand," and have probably a still higher coefficient of frictional resistance. The edgeways resistance offered by the blade is plainly analogous to that caused by its surface friction, and the same method of treatment will clearly apply to both circumstances; and obviously we are here dealing with an absorbent of power which cannot be left out of sight as inconsiderable.

But however confidently on the strength of known data we might assure ourselves of the great loss of power involved in surface friction, we could not thereby arrive at any definite data as to the pattern and dimensions of the screw which would on the whole minimize the waste of power, unless we could bring also into the calculation the correlated propulsive reaction. Now, the pressure or reaction of a fluid on an area moving obliquely through it, has not till lately been reduced to a true theoretical solution; and though it had come to be understood that the old law which made the pressure vary as the square of the sine of the obliquity was entirely in error, and that in the reality the resistance was pretty certainly in proportion to the simple power of the sine, it is only quite recently that the question has received a sound theoretical solution.

An eminent mathematician of the day, Lord Rayleigh, has determined the law on stream-line principles, rigorously so far as the pressure on the advancing surface is concerned, for a plane relatively narrow in the line of motion. According to his solution, if P be the normal pressure acting on the face of the plane $P = \frac{2\pi \sin\theta}{4 + \pi \sin\theta} P'$, where P' is the pressure of a head due to the speed, acting on the plane, and θ is the angle between the plane and the line of motion.

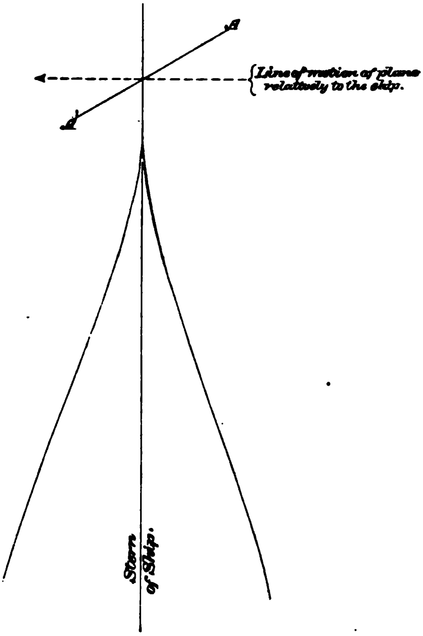
Lord Rayleigh regards it as a supposition *prima facie* admissible, that the water involved in the eddy at the back of the plane will probably possess a pressure equal to that of the undisturbed fluid, the assumption being that the disturbance created in the fluid by the passage of the plane will be obliterated, and no more than obliterated in eddy motion. It appears pretty conclusively, however, by Beaufoy's experiments that when the plane is moving normally through the water, so that $\theta = 90°$, the resistance actually experienced exceeds 'P in the ratio of 112 to 96, and it is not improbable that a proportionate excess, beyond P as given by Lord Rayleigh's formula, will be experienced also when the motion is oblique; and in the calculations I have made I have assumed this to be the case.

As regards surface friction, the experiments I have conducted for the admiralty show that it varies about as the power 1.85 or 1.9 of the speed; but for convenience we may adhere to the usual expression that it varies as the square of the speed. The coefficient or frictional force per square foot at unit speed, varies greatly with the length of the plane in the line of motion and with the quality of the surface.

The pressure and the friction may be respectively expressed with sufficient exactness by the equations $P = p\ A\ v^2 \sin\theta$, and $F = f\ A\ v^2$, where p and f are respectively the pressure and the friction per unit of surface, A the area of the plane, v the speed in the line of motion, and θ the angle between the plane and the line of motion; and if we take the forces in pounds, the area in square feet, and the speed in feet per second, the available data suggest 1.7 as the value of p, and 0.008 as the value of f, bearing in mind, as regards the latter figure, that it provides for the circumstance that the area of a screw-blade has a double surface, the back and the front, and that it is appropriate to a fairly smooth surface, measuring 3 feet in the line of motion. I must, however, add that although it is very important to be pretty correctly informed as to the true measure both of surface friction and of normal pressure, so as to be assured that we are dealing with real and tangible amounts and not with shadowy tendencies, the investigation, even when carried out with the mere abstract coefficients, proves in the highest degree interesting and instructive.

A true conception of the relation between the *modus operandi* of the obliquely moving plane and the blade of a screw propeller may be found by imagining the plane to be carried round the screw axis, by a perfectly strong radius of great length relatively to the size of the area, and

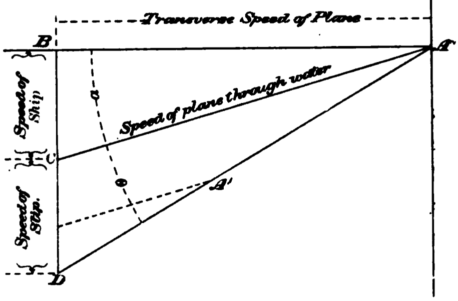
FIG. I.



of indefinitely small thickness, the plane being set obliquely to the plane of rotation, as if it were an unit of area in an extended true spiral surface. Thus viewed we see the action to be in principle the same as that of the action of a fish's tail, or of an oar used in sculling, although the action must be regarded as continuous instead of intermittent. And it is easy to see that the most essential of the dynamic elements of the action are simply those which would be in operation if a series of planes, the edgeway view of one of which is represented on plan by the line A A^1 in the adjoining sketch, were being continuously moved transversely, with a definite steady speed past the ship's stern, actuated by machinery within the ship; each plane in its turn taking up the whole duty and its predecessor being supposed to be annihilated Under these circumstances if the speed of the ship and the transverse speed of the planes is steady and continuous, a steady propulsive force is developed, and the speed will become steady also. In view of the elementary nature of the conditions to be elucidated, I shall disregard the ship's wake, and imagine the action to take place in wholly undisturbed water.

By the forward motion of the ship, the motion of the plane which is purely transverse as regards the ship, becomes obliquely forward as regards the water; and the directions and magnitudes of the several elementary motions involved, and the attitude of the plane in relation to them may be understood from the annexed diagram.

FIG. 2.



As before, the line A A′ being the plane, as seen edgeways in plan, A B is the direction and speed of the plane's transverse motion relatively to the ship; call this speed V: B C is the direction and speed of the ship's forward motion; call this speed v: A C is the direction and speed of the compound motion of the plane through the water; call this speed v'.

The difference between the direction of the plane itself, A D, and the direction of its motion through the water, A C, may be called the slip angle; and the speed of the slip taken account of in a direction parallel to the ship's motion is given by C D; call this speed s.

Call the angle B A C the virtual pitch angle, say, α.
Call the angle C A D the slip angle, say, θ.
Call the angle B A D the actual pitch angle, say, $(\alpha + \theta)$.

Then, in the usual terms of screw propulsion, the slip ratio is $= \frac{\mathrm{C\,D}}{\mathrm{B\,D}}$ or $\frac{s}{v + s}$.

If R be the propulsive force required to maintain the ship's speed, then, if we take dimensions in feet, and time in seconds, the useful or effective work in foot-pounds done per second is $R\,v$; and the problem to be solved is to apportion the area, and the angles α and θ, so as to make the effective work done bear the greatest ratio to the total work delivered to the propeller.

Besides the work of propulsion, there is the work done in surface friction, and for convenience this may be regarded as resolved into its longitudinal and transverse components. It may be observed that the longitudinal component is manifestly, in effect, an addition to the ship's resistance; so much more propulsive force is to be provided by reason of its operation.

The actions of the several forces with their respective speeds may be deduced and correlated by the regular mathemetical methods, so as to produce a fairly complete solution, and the steps of the solution are given in the appendix; it is sufficient here to mention the leading features of the results.

The area which will drive the ship with a given slip ratio, is directly as the ship's resistance and is inversely as the square of her speed. And since at moderate speeds a ship's resistance may be taken as proportional to the square of her speed, the same area of propeller will at all moderate speeds drive a given ship with the same slip ratio, and areas directly as the squares of the respective dimensions of two similar ships will drive each ship with the same slip ratio, since at such speeds for similar but differently dimensioned well-formed ships the area of wetted surface in each measures its resistance. At the higher speeds which introduce other elements of resistance, the slip ratio will become greater with the given propelling area. If two ships have the same resistance at different speeds, the area which will overcome the resistance while maintaining a given slip ratio, will be the less, in the ratio of the squares of the speeds, for the ship which has the higher speed.

The variation of area in terms of varying pitch and slip ratio, admits of no simple verbal statement. The difficulty is the greater since the relation of slip ratio to pitch and slip angle is itself, verbally, not very simple.

Thus far the results accord fairly with ordinary beliefs. The importance of the results shows itself chiefly when they are thrown into a form which expresses the efficiency of the propeller, or the ratio of the effective work realized in the ship's propulsion to the gross work delivered to the propeller.

It is obvious that if friction were zero we might secure perfect efficiency by making area infinite, for we could thus absolutely obliterate slip, whatever the pitch angle. That is to say, if we could disregard the limitation which practical considerations require in the area of the propelling plane, the obliqueness with which it acts on the water would cause no loss of efficiency.

Graphic treatment of the equation of efficiency quickly shows that, though the maximum of efficiency is not produced by extending the area of propelling plane so as to minimize slip, yet that, on the other hand, the slip angle which gives maximum efficiency is moderate—that is to say, so moderate that no serious error is introduced by regarding the sine of the angle as equal to the arc. And this change materially simplifies the equation, rendering it easy to treat by the usual method of maxima and minima, so as to determine what slip angle and what pitch angle secure the maximum of efficiency.

We thus are led to the conclusion that the value of θ, which gives the maximum efficiency, is the same whatever be the value of $a + \theta$ the pitch angle. In fact, to make the efficiency a maximum the slip angle must be directly as the square root of the co-efficient of surface friction, and inversely as the square root of the co-efficient of normal pressure;

the value is $\theta = \sqrt{\frac{f}{p}}$.

It will be at once seen, from the geometry of the case, that although the slip angle ought thus to have the same value whatever be the pitch angle, the slip ratio which results will be much greater for large pitch gngles than for small ones.

A still more curious and interesting conclusion is that which defines the pitch angle of maximum efficiency. If the slip angle be that which gives the maximum efficiency, then to produce the maximum efficiency the propelling plane ought to stand at an angle of 45° with the line of the ship's motion; and this equally happens, whatever be the co-efficients of surface friction, or of normal pressure.

The rationale of the conclusion is, that whereas, if the skin friction of the area is thrown out of the account, the efficiency would be theoretically perfect, as already explained (since the operative area might be indefinitely extended and the pitch would be indifferent), yet when account is taken of surface friction, the pitch ceases to be a matter of indifference, and if the propelling area is so apportioned as to give the slip angle appropriate to the co-efficients of normal pressure and of skin friction, the combination of the conditions in virtue of which the friction diminishes efficiency proves to operate with greater aggregate effect at any other pitch angle than 45°; for if, on the one hand, the pitch angle be increased beyond 45°, the loss is increased in virtue of the friction of the excess of area then needed to maintain the propulsive force with the given slip faster than it is decreased by the reduced speed at which the friction operates; if, on the other hand, the pitch angle be reduced below 45°, the loss is increased in virtue of the excess of speed with which the friction operates more than it is decreased by the diminution of area necessary to maintain the propulsive force with the given slip.

If the slip angle exceed that which gives the maximum efficiency, the pitch angle must also be increased; if the excess be small, the pitch angle must be increased by the same amount; if the excess be large, the increment of the pitch angle must be still greater.

Taking as the co-efficients of normal pressure and of surface friction those already mentioned, the slip angle of maximum efficiency gives a slip ratio of about 12½ per cent., and the maximum efficiency realized is about 77 per cent. If the area is so apportioned as to give either a greater or a smaller slip ratio the efficiency will be diminished, but the diminution is much more rapid if the slip ratio is diminished than if it is increased, as may be seen by referring to the diagram (Fig. 3, Plate II.) The diagram (Fig. 4, Plate II) shows how the efficiency varies in terms of the pitch angle of which the pitch ratio is the tangent. It will be seen that a given defect of pitch ratio or tangent of pitch angle injures efficiency considerably more than an equal excess of it.

The most salient conclusions to which we are led by this review of the elementary principles on which the efficiency of the screw-propeller depends are (1), that a very much longer pitch than has been commonly adopted is favorable to efficiency; and (2), that instead of its being correct to regard a large amount of slip as a proof of waste of power, the opposite conclusion is the true one. To assert that a screw works with unusually little slip, is to give a proof that it is working with a large waste of power.

In applying to the operation of the twisted surface of an actual screw-blade the principles which have been traced in relation to the operation of a plane area, no serious difficulty presents itself; but considerations of considerable intricacy are involved in dealing with the very complex circumstance that the water in which actual screws work has been disturbed by the passage of the ship, and has been converted into a current which more or less rapidly follows her. This circumstance is of very high importance in several respects, but I shall not here refer to it further than to say that in practice, since one certain effect of it is to make the slip ratio appear smaller than it really is, the conclusion that a greatly reduced slip is a proof of waste of power has been largely masked, and the difficulties of an empirical solution of the problem both as regards slip and pitch have been greatly augmented.

It may be useful to observe in conclusion, that whatever may be the effect of the difficulties just referred to as attaching to the extension of the solution from the action of the obliquely propelling plane to that of an actual screw, there are two assertions which may be confidently made in reference to the investigation and its results (1), that the conclusions which have been drawn as regards the plane are in substance incontestable, so far as concerns their character and general bearings; though it is probable that quantitatively they may need some correction on the score of the incomplete exactness of the co-efficients of pressure and of friction, which have been provisionally suggested; and 2, that no theoretical treatment of the action of an actual screw can be sound which does not incorporate and mainly rest on the principles embodied in the treatment of the problem of the plane, and indeed that the character of the results must, in their most essential features, be the same in both cases.

MATHEMATICAL APPENDIX.

Taking the construction given in Fig. 2, p. 50, as the geometrical basis of the solution, A A′ is the propelling plane, the area of which is A. The path of the plane relatively to the ship is transverse to her path and parallel to the line A B, and its speed V. The path of the ship relatively to the water is parallel to B C, and her speed v. The path of the plane relatively to the water is therefore the resultant of the two motions, and is parallel to A C, and its (resultant) speed v'. Call the angle B A C or the "virtual pitch angle" α; the angle C A D, or the angle of the plane relatively to its path, θ; thus the angle B A D, or the "actual pitch angle," $= \alpha + \theta$. Pressures will be expressed in pounds, speeds in feet per second, areas in square feet, angles in circular measure.

As has already been explained in the text, the normal pressure on the plane area moving through the water with any speed v, on a path forming the angle θ with the plane is $P = p\,A\,v^2 \sin\theta$ (p = about 1.7). The surface friction on an area moving edgeways through the water is $F = f\,A\,v^2$ (for an area having a length in line of motion equal to about the middle of an average screw-blade, the co-efficient of skin friction is about 0.004, and since A has two surfaces $f = .008$). In the case under consideration—

$$P = p\,A\,V^2 \sec^2\alpha \sin\theta$$
$$F = f\,A\,V^2 \sec^2\alpha$$

The effect of P in the line of the ship's motion $= P\cos(\alpha + \theta)$, but from this must be deducted the longitudinal component of the surface friction, which $= F \sin \alpha$.* Thus the net force available for propulsion $= P \cos (\alpha + \theta) - F \sin \alpha$.

This propulsive force must equal the ship's resistance, R. Hence we have the equation $R = P \cos (\alpha + \theta) - F \sin \alpha$.

The effective work done is that employed in overcoming the ship's resistance with the ship's actual speed. Call this work U_e; then $U_e = R\,v = R\,V \tan \alpha$; and making the requisite substitutions, and putting k for $\frac{f}{p}$, we obtain the equation.

$$U_e = p\,A\,V^3 \sec^2 \alpha \tan \alpha\,[\cos (\alpha + \theta) \sin \theta - k \sin \alpha] \quad . \; . \; . \; . \quad (1.)$$

The gross work done is the transverse component of P + the transverse component of the surface friction, each acting with the speed V; call the gross work U_g,

$$\text{Then } U_g = p\,A\,V^3 \sec.^2 \alpha \left\{ \sin (\alpha + \theta) \sin \theta + k \cos \alpha \right\} \quad . \; . \; . \; . \quad (2.)$$

If, for convenience, we prefer to express U_e and U_g in terms of the ship's speed v, instead of in terms of the transverse speed of the plane in relation to the ship or V, remembering that $v = V \tan \alpha$,

$$\text{We have } U_e = p\,A\,v^3 \operatorname{cosec}^2 \alpha \left\{ \cos (\alpha + \theta) \sin \theta - k \sin \alpha \right\} \quad . \quad (1a.)$$

$$U_g = p\,A\,v^3 \operatorname{cosec}^2 \alpha \cot \alpha \left\{ \sin (\alpha + \theta) \sin \theta + k \cos \alpha \right\} \quad . \quad (2a.)$$

But also $U_e = R\,v$, and this substituted in (1*a*) gives the value of A necessary to maintain propulsion.

$$A = \frac{R}{p\,v^2 \operatorname{cosec}^2 \alpha \left\{ \cos (\alpha + \theta) \sin \theta - k \sin \alpha \right\}} \quad . \quad (3.)$$

And substituting this in (2*a*) we have

$$U_g = R\,v \cot \alpha \frac{\sin (\alpha + \theta) \sin \theta + k \cos \alpha}{\cos (\alpha + \theta) \sin \theta - k \sin \alpha} \quad . \; . \; . \quad (2b.)$$

We can now frame the equation of efficiency say E

$$\text{For, } E \left(= \frac{U_e}{U_g} \right) = \tan \alpha \frac{\cos (\alpha + \theta) \sin \theta - k \sin \alpha}{\sin (\alpha + \theta) \sin \theta - k \cos \alpha} \quad . \; . \quad (4.)$$

This equation, if differentiated for θ and $\tan \alpha$ respectively, would enable us to determine by the usual process the values of θ and $\tan \alpha$ which would respectively and jointly give the greatest value to E; but the differentiation does not lead us to a soluble equation; on the other hand, a few successive graphic solutions of the equation of efficiency show that the value of θ which gives maximum efficiency, is under all ordinary conditions a small one—so small that no appreciable error will result from taking sine $\theta = \theta$, and cos $\theta = 1$. With these substitutions the equation (4) becomes,

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* It might at first sight be assumed that this component should be taken account of in the direction of the plane, not of the motion of the plane; but it appears on consideration that all the particles to which the plane frictionally imparts motion along its own plane, must accept at the same time the normal component of the plane's motion, and thus its complete resultant path; the force should therefore be estimated as acting in the direction of the resultant motion of which it is the counterpart. [Since the above matter was in type I have been led to doubt the correctness of this assumption, and to lean to what was my original impression, that the component should be taken in the direction of the plane itself; but the assumption simplifies the solution, and the principal results arrived at are not materially affected by the slight error it involves, as the whole work of skin friction is included under either hypothesis. I had traced the solution far enough under my original impression to know that the more complete solution which I retain as already in type is practically admissible.—W. F.]

$$E = \tan\alpha\,\frac{\theta\cos\alpha - (\theta^2 + k)\sin\alpha}{\theta\sin\alpha + (\theta^2 + k)\cos\alpha} = \tan\alpha\,\frac{\theta - (\theta^2 + k)\tan\alpha}{\theta\tan\alpha + (\theta^2 + k)} \quad . \quad (4a.)$$

The two differentiations of this equation give respectively the following values of θ and tan α as the conditions of maximum efficiency.

When θ varies independently

$$\theta = \sqrt{k} \quad \ldots\ldots\ldots\ldots \quad (5.)$$

When tan α varies independently

$$\tan 2\alpha = \frac{\theta}{\theta^2 + k} \quad \ldots\ldots\ldots\ldots \quad (6.)$$

$$\text{or } \tan\alpha = \sqrt{1 + \left(\theta + \frac{k}{\theta}\right)^2} - \left(\theta + \frac{k}{\theta}\right)^{*} \quad . \; . \quad (6a.)$$

With this value of tan α it easily follows that—

$$\tan(\alpha + \theta) = \frac{\sqrt{\theta^2 + (\theta^2 + k)^2} - (\theta^2 + k) + \theta\tan\theta}{\theta - \theta\tan\theta\left[\sqrt{\theta^2 + (\theta^2 + k)^2} - (\theta^2 + k)\right]}$$

And substituting in this form (5) we find as the condition of maximum efficiency on the whole—

$$\tan(\alpha + \theta) = \frac{\sqrt{1 + (2\theta)^2} - 2\theta + \tan\theta}{1 + 2\theta\tan\theta - \tan\theta\sqrt{1 + (2\theta)^2}} \quad . \; . \quad (7.)$$

Now, observing that when θ is small, tan θ does not exceed θ by $\frac{1}{3\theta^3}$, so that when $\theta =$ (say $\frac{1}{5}$), or just exceeds 10°, the difference between the angle and the tangent is less than $\frac{1}{375}$, we may put tan $\theta = \theta$, and since within the same scale of approximation $\sqrt{1 + (2\theta)^2} = 1 + 2\theta^2$, we have—

$$\tan(\alpha + \theta) = \frac{1 - \theta + 2\theta^2}{1 - \theta + 2\theta^2 - 2\theta^3}, \text{ or in effect} = 1. \quad \ldots \quad (7\,a.)$$

Thus in all cases where the due relation between θ and k is maintained so as to secure maximum efficiency, the actual pitch angle which will give the maximum efficiency is 45°.

In view of the probability that slip will be either greater or less than that which corresponds with maximum efficiency, it is desirable to see what modification of pitch angle will tend to minimize the loss of efficiency occasioned by the excess or defect of slip.

This is already deducible from the equation for tan α in terms of θ, but it is in some respects more convenient to shape the equation in

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[*] The term $\left(\theta + \frac{k}{\theta}\right)$ is necessarily so small when $\theta = k$, as in equation (5), that the equation is practically equivalent to $\tan\alpha = 1 + \frac{\left(\theta + \frac{k}{\theta}\right)^2}{2} - \left(\theta + \frac{k}{\theta}\right)$

such a form as will show the departure which the angle $(\alpha + \theta)$ should, under the circumstances, make from 45°, which is that which gives the complete maximum of efficiency.

Call the slip angle of maximum efficiency θ, $(= \sqrt{k})$,

And the slip angle θ.

If we call $\alpha = 45^\circ - \gamma$, then $\alpha + \theta = 45^\circ - \gamma + {}'\theta$, and

$$\tan \alpha = \frac{1 - \tan \gamma}{1 + \tan \gamma}$$

And since we shall not deal with large values of γ, we may with sufficient approximation take $\tan \gamma = \gamma$, so that $\frac{1 - \gamma}{1 + \gamma} = 1 - 2\gamma + 2\gamma^2$.

Making these substitutions, and putting for tan α its general value[*] of maximum efficiency, we have—

$$1 - 2\gamma + 2\gamma^2 = 1 + \frac{\left(\theta + \frac{k}{\theta}\right)^2}{2} - \left(\theta + \frac{k}{\theta}\right)$$

$$\text{Or } \gamma^2 - \gamma = - \frac{\left(\theta + \frac{k}{\theta}\right)^2}{2} + \left(\frac{\theta + \frac{k}{\theta}}{2}\right)^2$$

$$= - \frac{\theta + \frac{'\theta^2}{\theta}}{2} + \left(\frac{\theta + \frac{'\theta^2}{\theta}}{2}\right)^2 \text{since } k = {}'\theta^2$$

$$\gamma = \tfrac{1}{2} - \overline{\tfrac{1}{4} \left\{1 - 4\left(\frac{\theta + \frac{'\theta^2}{\theta}}{2} - \left[\frac{\theta + \frac{'\theta^2}{\theta}}{2}\right]^2\right)\right\}}$$

If we expand the quadratic root by the binomial theorem, and take the first three terms of the series, dropping the third and higher powers of θ, we obtain the result with a very close approximation, at least with such moderate values of θ as are here contemplated, since it is desired chiefly to determine the initial change which should be made in $(\alpha + \theta)$ to secure maximum efficiency when θ has differed, but has not differed much, from $'\theta$. Taking the solution thus, and reducing, we obtain—

$$\gamma = \tfrac{1}{2}\left(\theta + \frac{\theta'^2}{\theta}\right)$$

And if we call $\theta = \theta' (1 + h)$ where h is some moderate increment whether positive or negative, we obtain—

$$\gamma = \theta^1 \left(1 + \frac{h^2}{2} - \frac{h^3}{2} + \frac{h^4}{2} \text{ \&c.}\right)$$

And for the angle of pitch—

$$(\alpha + \theta)\ (= 45^\circ - \gamma + \theta) = 45^0 + \theta^1 \left(h - \frac{h^2}{2} + \frac{h^3}{2} \text{ \&c.}\right) \quad . \quad . \quad . \quad . \quad (7b.)$$

That is to say, any moderate alteration of slip angle would demand that to give maximum efficiency, the pitch angle should receive an increment or decrement in effect equal to that of the slip angle.

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[*] See foot-note to equation (6a).

We shall determine the complete maximum efficiency, say 'E, by introducing into the equation of efficiency the appropriate values of tan a and θ.

The substitutions give—

$$E' = 1 - 2\sqrt{k} + 2k\frac{1 - 2\sqrt{k}(1 - 2\sqrt{k} + 2k)}{1 + 2k} \quad . \; . \; . \; . \; . \; . \; . \; . \quad (8.)$$

Or very approximately $= 1 - 4\sqrt{k} + 8k - 8k\sqrt{k}$. . . . (8*a*.)

If we adopt the value of k, which has been suggested, or $k = 0.0047$, and introduce it into this equation, we obtain $E' = 0.77$; that is to say, if we adopt the most probable values of p and f, and assume the propelling plane to be placed at the most effective pitch angle, and to operate with the most effective slip, 77 per cent. of the work delivered to the plane is the utmost that can be realized in propulsive effect. The efficiency of a screw cannot be so great as this, since only one region of the blade can have the most effective pitch angle.

We can also assign the value of A, say A′, which will secure maximum efficiency, the other necessary conditions being complied with.

If the ship's speed be v and her resistance R, then the area

$$A' = \frac{R}{v^2} \; \frac{\sin^2 a}{\cos(a + \theta)\sin\theta - k\sin a} \quad . \; . \; . \; . \; . \; . \; . \quad (9.)$$

We may substitute here as the conditions of maximum efficiency $(\theta + \theta) = 45^\circ$, $a = 45^\circ, - \theta$, $\theta = \sqrt{k}$, and making the reductions, the equation approximately becomes—

$$A' = \frac{R}{v^2}\left(\frac{1}{\sqrt{k}} - 1 - 2\sqrt{k}\right) \quad . \; . \; . \; . \; . \quad (9a.)$$

Which valued out, assuming as before that $k = .0047$, gives $A' = \frac{R}{v^2} \times 8.9$.

To give a practical conception of the result, we may take the case of a ship whose resistance at 17 knots is 40,000 pounds, which corresponds nearly with the case of H. M. S. *Iris*. Applying these data, the area required is 405 square feet, the slip angle being 3° 56′ 30″, and the slip ratio 12¾ per cent.

The efficiency would be as given above, 77 per cent., but it is deserving of note that in the first place the area required is nearly inversely as the slip assigned, and as efficiency decreases but slowly when the slip is increased beyond that due to the maximum efficiency, a greatly reduced area would be admissible without much loss of efficiency.*

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* Experiments, which have been in progress since this paper has been in type, show conclusively that the decrease of efficiency consequent on increased slip, with screws of ordinary proportion, is scarcely perceptible even when the slip ratio is as large as 30 per cent., with the screw working in undisturbed water. The results so shaped themselves as to point to the conclusion that for some reason or other, the co-efficient of surface friction began to diminish when the slip ratio became as much as 15 per cent., and was about halved when the slip ratio was 30 per cent.; and as it appeared

DISCUSSION.

Mr. **John Scott Russell**, F. R. S. (vice-president). My lord, I rise to make some remarks on Mr. White's paper, in regard to which I feel that both I and all the members of the profession are extremely and exceptionally indebted to him. A good many years ago I was a member of a committee of the British Association who communicated with the admiralty on the desirability of having such experiments very carefully conducted, and extremely carefully analyzed. And now I must say that this is one of those papers and communications to our profession which answers to all the purposes for which the committee of the British Association was formed, and for which that committee applied to the admiralty. I consider this a most valuable paper. Now, I think, although we are most of us very fond of making our own little experiments, and little models, yet after all we derive infinitely more instruction from experiments made with this precision on full-sized ships than we do from small models. In regard to the question of two screws against one, there is one most important point to which all we practical men attach great value, and that is, that two screws propel better than one; and the practical point is this, that in all machinery, especially machinery of war, it is of the greatest consequence to have duplicates; that in every portion of the critical machinery of a ship of war we should have a second stand-by when an accident has happened to one. Two screws give this valuable quality in which one is wanting, therefore there is a great argument in favor of two screws. There is another point in which I think two screws may have a great advantage, which has not been alluded to in the paper. There is in every ship a most favorable place in which to put a screw; and the serious point is, where is that most favorable place? If you have two screws, you may put the screw-propeller considerably forward on the body, and if you have one screw you may put that screw considerably aft on the body; and the question is, whether the two smaller screws placed forward on the body, or the single larger screw placed aft on the body, are the better propellers. Allow me to call your attention to this most important fact. You should study the speed at which that vessel is to be propelled, and its reference to her form. You all know that there is near the stern of the ship what we call a wave of replacement, which has the following qualities—either that it comes in a good position in reference to the screw, or it comes in a bad one; and the question in regard to each ship is, whether you will place the screw immediately under the crest of this wave, or whether you place it

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not improbable that with increasing slip a more or less pronounced eddy might become established at the back of the blade, so as more or less completely to neutralize the friction on that surface, a rough experiment was tried by moving a plane obliquely through the water with various angles of slip, and in a position where the effect could be observed; and in point of fact it appeared that when the angle between the plane and its line of motion was about 10°, the water at its back had assumed the form of an eddy, having nearly the speed of the plane, and that it in fact overran the plane when the angle was increased to 15°.

in the bad position with the hollow of the wave near it or above it. Now, allow me to tell you the cause of this difference. On the crest of that wave the water is going forward in the direction of the ship, and contrary to the slip of the screw. In this wave, in the same manner, at the top the water in the crest is going rapidly forward in the direction of the ship, and consequently is opposed to the screw; therefore, in these two positions the slip of the screw is greatly diminished. But in the hollow, on the contrary, the water is going in the opposite direction, and thereby the slip of the screw is greatly increased. In my opinion, where the ship is properly formed, it is so formed that at the given speed the crest of that wave will be in the plane of the screw, and then there will be a minimum slip; and I think that the efficiency of the twin-screws will be found to be very materially dependent on having chosen for them the best place, and where those twin screws have proved a failure it will probably be found to be because they were put in the wrong place. What that place is you all know who have studied the wave system, because you know there is an exact point at which, with a given speed, the crest of the summit of the wave of replacement will be found. The one will be a good form for the twin screw, and the other will be a good form for the single screw. One point more in favor of the double screw is, that in pitching, as the screw is a good deal lower under water it will not be exposed so much to the inequalities as the other.

Mr. CHARLES W. MERRIFIELD, F. R. S. (associate member of council). My lord, I think the meeting will agree with me that Mr. White's paper thoroughly deserves the eulogy passed on it by Mr. Scott Russell, as being a thoroughly painstaking investigation of actual facts, and not dealing largely with generalities, but where it does use any, supporting them very carefully by an actual qualitative discussion of the performances of different ships of full size. I think it would be impossible to overestimate the value of such papers to this Institution and to the science of naval architecture. But there is one point in which I was unable to follow my friend Mr. White, and that is in the application of those figures to the general object of the paper. As I understood him, he was dissatisfied with the statement commonly made, that because in a vessel of large draught you can get a large propeller area, there is an advantage in vessels of large draught in having a single screw, and for an opposite reason in vessels of small draught in having a double screw. Now, being thus very reasonably dissatisfied with a general statement of that kind, he has done a very rational and useful service in going into it qualitatively, and examining the relative performances of vessels of deep draught under varied conditions. But when he comes to the general conclusion, which is the object of the paper, and which he deduces from these figures, I am a little unable to follow him in this respeet, that when I look to the proportions of the vessels he examines, I find that although these admiralty vessels are without any exception of deep draught, taken absolutely, yet when I take these vessels relatively to their beam I find they are all vessels of proportionately shallow draught. I find that with scarcely an exception, and I think without an exception, the draught is considerably less than half the beam, in some cases as little as two-fifths of the beam. Now, in most cases of deep merchant ships the proportion is quite as often three-fifths as one-half the draught. I cannot quite follow Mr. White in his conclusion that because with a vessel of absolutely deep draught, but of relatively shallow draught, the twin-screw propeller is an advantage over the single propeller (which was a conclusion to be expected from the ordinary reasoning applied to the general case), that the twin-screw propeller is on the whole preferable; and it does not seem to me quite possible to make out his case of the general advantage in merchant ships, in which the proportions are different, of having twin screws instead of one, without examining the case where twin screws have been successfully applied to a vessel of deep relative draught—that is to say, something about three-fifths of her beam. That is, of course, an important point, which Mr. White will be able to deal with in his reply, but I think it is important that we should not adopt his conclusions without at least paying attention to it.

Mr. E. J. REED, C. B., F. R. S., M. P. (vice-president). My lord, I would wish to make one or two remarks on this paper, and I would first refer to the *Iris* because I was very glad indeed to see Mr. White bring the *Iris* into this investigation. I believe, without the slightest blame to anybody, she is, with regard to her great object of being a fast ship furnished with twin screws, a signal failure up to this time—that is to say, she is a ship performing nothing like the speed which is due to her power in contrast with other ships. I say that not at all with a view to cast a reflection upon anybody, because I do not think there is anybody to cast a reflection upon. I think the course taken as stated in the paper was a justification for the admiralty trying the experiment. It was a great experiment. An enormous speed was desirable from a vessel with moderate dimensions, and from the draught of water which alone could be allowed you could only put a single screw with a diameter out of all proportion to the power to be exerted according to our previous experience. That being the case, the admiralty wisely adopted the twin screws. They put enormous power on them, and the result has thus far been a great failure, the ship showing nothing like the speed intended or expected under ordinary conditions. Therefore, I take it for granted that the *Iris* will be watched by all the members of this Institution with the deepest interest, because no doubt her case will prove pregnant, when further experiments have been made upon her, with information which we should very much appreciate.

The first experiment that has been made appears to have been to take away the four-bladed screw, and to put two-bladed screws, and the result has been, as I understand it, that with these two-bladed screws, and for a less development of indicated horse-power, the speed hitherto obtained is the maximum that has been secured. So that there is at present in the ship a reserve of horse-power which may be recorded, I think, at 1,000, but of which the corresponding speed has yet to be got. The members of this Institution will see in a moment that that is an experiment with a twin-screw ship of the greatest interest and importance, and all the more because of the suggestion of the fact to which Mr. Merrifield referred—namely, that these ships, the results of which we have tabulated, do not present a case analogous to the *Iris*, which is an instance more analogous to mercantile steamers, because her proportions are more like those vessels. These vessels (in Mr. White's table) are all of very great beam, and consequently, although their draught is great, the proportion of breadth to draught is very considerable. Mr. White came to the conclusion that the one serious objection to twin-screw ships was the danger of the screws getting injured. I must say we have experience at this moment which strongly confirms that, because I have just sent three ships to Japan through the Suez Canal, all of equal draught, and the two single-screw ships went through without the slightest difficulty, while the twin-screw ship knocked her propellers about in a very sad manner. That is a matter, especially with regard to ships likely to be sent to India through the Suez Canal, of the greatest possible importance. My lord, Mr. Scott Russell, pointed out the value of studying the position of the wave of replacement, and placing the screw in proper reference to it. But Mr. Scott Russell stated, as he was sure to state with his knowledge of the facts, that before you can fix your wave of replacement you must fix the speed of your vessel. I cannot understand myself how, for general purposes, you can fix your vessel's speed—at any rate, the speed of a war vessel—because, as you well know, my lord, we aim in war vessels at a great maximum speed, and the moment we have the ship with a very great maximum speed, we begin to use her, never, or very seldom, indeed, resorting to that maximum speed and making use of it. If you are sending a 13-knot vessel round the world, you never think of consuming the fuel required for the development of her maximum horse-power, but, on the contrary, you immediately reduce the consumption of fuel very largely, and send her on her actual service at a speed which has no relation whatever, so to speak, to her maximum speed. Therefore it is important, if we have to fix the position of the screw with relation to the wave of replacement, to know what speed we are to select, whether the 13 knots at the measured mile, which we are seldom required to do in the lifetime of a ship—but which when you wish to get it is of the greatest possible importance—or the average speed which we use in the ordinary service of the vessel. That is a point which I mention because I am satisfied that most of us who heard Mr. Froude's paper a year or two ago with reference to the position of the wave of replacement and its bearings on forms and proportions of vessels, will connect that valuable paper with the remarks of Mr. Scott Russell, and will see that the point involved is one of the greatest importance, but the difficulty of applying it is also I think extremely great, because of the varying speed at which ships are impelled. If Mr. Scott Russell could throw some light on that point I should be glad. There is one other remark which I wish to make bearing on these experiments, although I do not think it has any great weight. You will observe, on looking at Mr. White's tables, that the differences in the number of revolutions of the engines are not generally enormous, but I believe that our indicated horse-power, in all our returns, is liable to be in fault in the matter of revolutions. We only time our revolutions in units, and I, myself, on measured-mile trials, have noticed marked discrepancies between the revolutions taken in the engine-room and used in the calculations of indicated horse-power afterwards, and the revolutions taken on deck by myself and my officers. Why I mention it is this: If you take a ship developing her full power with a small number of revolutions, and make a difference of 1 revolution, or even 2, as I have known, of the engines, and you add 2 revolutions of the engines making 50, you add enormously to the indicated horse-power as recorded; whereas, if you add 2 with the engines going, as some of the vessel's engines were going—I see 81 in the case of the *Iron Duke*—you do not extract the indicated horse-power in anything like the same proportion. I think that Mr. White has been so careful and so wise, in my humble opinion, in fencing all his statements with a proper guard, that we have not the slightest fault to find with the paper; but, on the other hand, if any one should be disposed to make close comparisons between pairs of ships, I think that suggestion is one of considerable importance. I want to make one other remark, and that is that, for some reason or other—I do not know quite how it happens—we are getting, I think, rather too few of the results of the trials of Her Majesty's ships. It is very ungracious to say that at a moment when Mr. White, an admiralty officer, has given us these valuable records, but still I think it is the proper moment to mention that circumstance. Many years ago—I think your lordship was in power at the time—I remember, when quite a private person, asking that we might have the records of the trials of Her Majesty's steamships placed before the profession and the public, and the whole of them were given to me at that time for publication in the Mechanics' Magazine, which I was then associated with. Since that time we have had occasionally in our own transactions and other places the admiralty records put before us, but I would venture to suggest that we have not had for a long time connected and full reports of the trials of Her Majesty's ships; and while that fact enhances the value of those we do get, yet I think it worth the consideration of this Institution whether we could not ask the admiralty to give us the results of the trials of Her Majesty's ships a little more freely than they have been doing of late years. Perhaps the only reason is that they have not been asked for them, but if that is so, the fault rests with us. One thing is certain, that if the trials of Her Majesty's ships are made under conditions so free, comparatively, from the exaggerations which attend the trials of many other vessels, they would be of peculiar value in these days when the admiralty are building ships of such diversified types and such varying qualities; it would be a very great advantage indeed, to naval architects and to marine engineers to have the trials of Her Majesty's ships more fully in their possession.

Sir **ROBERT SPENCER ROBINSON**, K. C. B., F. R. S., admiral (vice-president). I only rise to supplement what my friend Mr. Reed has said about the non-publication of admiralty trials. It is in the interests of this Institution, both of the engineers and of the designers of ships, that the fullest publicity should be given to those trials, and he has not at all exaggerated the remarkable reticence of the admiralty for several years past. When he and I were connected with admiralty matters, every year, as regularly as the time came round, the whole results of the admiralty trials during the twelve months were printed and tabulated in the most minute form, giving a vast amount of very valuable information, and besides the official people connected with dock-yards and ship-building, one hundred or one hundred and fifty copies were distributed amongst those people who had most interest in the subject, and in fact anybody who chose to apply for a copy of those trials received it. Since 1872 no such publication has been made—there has been no calculation accessible to the public, to ship-builders, or to the makers of engines; and I could not, while listening to the very valuable paper Mr. White has put before us, considering the extreme interest of the information contained in it, and the scope of the argument which he endeavored to adduce in favor of twin screws, help feeling that it was a very great misfortune to every one that the results of the admiralty trials had not been circulated in the same way as they had been in previous years. If it were not an unkind or an ill-natured thing to say, one almost might have supposed that the non-publication of these trial was with the view of concealing certain unfortunate failures that had taken place, but I believe that no such motives could influence the judgment of the officers of the admiralty in withholding these valuable results from us. I have several times been asked why the publication of this information has been withheld from us, and the suggestion that I have just hinted at has been made, than which I think nothing more illiberal or ill-natured could have been said. In the interest, therefore, of those public officers who work so hard and who deserve so much encouragement from society and from every member of this Institution, I trust that this reticence in publishing important facts which influence the construction of ships and the building and trial of engines will no longer be maintained, and that we shall have the fullest information, as we have had in past years, that the admiralty can supply.

Mr. **JAMES WRIGHT** (vice-president). I should like to say a few words, not with reference to Mr. White's paper generally, because he has so fully stated the subject both for and against the twin screws, but with reference to the *Iris*, and more particularly with regard to Mr. Reed's remarks. He has characterized the *Iris* as a failure. To a certain extent she is, so far as her speed is concerned.

Mr. E. J. **Reed**. That is all I said.

Mr. **Wright**. That failure is not in any measure due to the hull or to the engines. So far as we can see at present, and I think those who have considered her trials will bear me out in saying this, it is due to the screw only—she is very much overscrewed. The two-bladed screw was simply the same screw as the four-bladed one with the two intermediate blades taken off. These experiments were made in connection with Mr. Froude, and it was desired to get the results as nearly under the same conditions as possible to determine the friction and resistance. If you follow out the lower curve you will see that with 7,000 horse-power she would go about 18 knots, and that is more than was expected, because she was only expected to go between 17 and 17½. With the two-bladed screw, if the blades were strong enough, so that the whole power of the engines could be put out on them, no doubt the full speed expected from the *Iris* would have been obtained. It is only right for me, who was chiefly responsible for the design of the screw, to clear my colleagues from any blame as regards the ship, and as regards the performance of the engines nothing could be better. Improving the screw is a very simple matter, and very easily done. I regret to say I know very little of the conditions under which the twin screws in these very high-powered ships work. I hope to be able to get some experiments made with the *Iris* with the view of getting data for future guidance, and I hope the results of those experiments will be laid before the Institution for the information of the members. Just one other remark with regard to what Mr. Reed said about the revolutions and the manner in which the engine-power was calculated. I should have thought Mr. Reed would have given us more credit for that than he has done. As a matter of fact the revolutions are taken on the measured mile by two counters. There is one man set to each counter and the revolutions are taken independently. If they do not agree the average of the two is taken. By that means, and by taking the revolutions over the whole run of a mile, I think it must be allowed there is very little chance of error, to even the very small fraction of a revolution. There is one other thing I should like to give an explanation of, and that is Sir Spencer Robinson's remarks about the tables of trials. A number of years ago, when Sir Spencer Robinson was at the admiralty, an order was given that these trials should be considered as confidential; and as we all know admiralty traditions are rather lasting sometimes, that order has never been removed, but I have no doubt if the matter were represented in a proper manner there would be no difficulty in getting the order removed, and that the papers as published would be given, as was the case formerly.

Mr. **William Denny** (member of Council). I wish to support the request of Mr. Reed, because in the north the wish is often expressed that we should have the results of the admiralty trials. I must say that I never heard such a suspicion as Sir Spencer Robinson has mentioned that he heard mooted on the subject. The only thing I have heard is, what is my own wish, that the admiralty, having benefited directly and indirectly the mercantile marine in many ways, might add to that benefit information with regard to all their trials. There is a great deal of this information contained in Mr. Scott Russell's book, but we wish we had it completed. One other remark, and that is with regard to the danger of twin screws being broken. I think Mr. Reed somewhat overrates that. It is quite true that the Suez Canal is a very crucial place for twin screws, but there is a large amount of water in the world which is not the Suez Canal. On the Clyde twin screws are at present being used for tug-boats, and of all uses you would fancy that to be one which would subject them to very great risk; but that risk has been entirely obviated by the breadth of the vessel on the deck being carried to the full extent aft to the stern, so that really the stern overlaps each of the twin screws to the same extent as an ordinary stern would overlap a single screw. I thought it worth while calling attention to this, because difficulties of this kind should not be feared too much if they are really capable of being overcome.

Mr. **John R. Ravenhill** (member of Council). I will ask the indulgence of the meeting for a few minutes, my name having been mentioned in this paper. First, I should like to remark that Mr. White tells us of the great improvement in the speed of the *Invincible* from shifting the position of the screw in the fore-and-aft line of the vessel.

Mr. **W. W. White**. And altering the pitch.

Mr. **J. R. Ravenhill**. Yes, and also altering the pitch. In the case of the *Vanguard*, the sister ship, there was a considerable difference between the vertical center line of her propellers and the rudder-post; there was a very considerable difference between those lines in the *Iron Duke*, another sister ship, and in the *Vanguard*; and I know, at the time, the admiralty authorities considered that the increased speed of the *Vanguard* was to some extent to be attributed to it. It would increase the value of Mr. White's paper, I think, if he could just add a column giving us the relative positions of the screws in the way I describe. As regards the *Avni Illah* and the *Muini Zaffer*, it was a trial made some years back, but very carefully conducted, and there was one very great advantage in that trial as far as comparison went. I am now speaking of the results obtained by the machinery. The boilers were identically the same, and it is to your boilers that you must look for your power. There was a very great difference between the disk area of the twin screws, in proportion to the midship area of the vessel, and in that of the single screw.* There is also another point connected, I think, with vessels fitted with double screws which nobody has alluded to to-day, and that is this—anybody who sees a vessel in dock must be struck with the fine lines aft that can be continued to the rudder-post, as against the form that the ship-builders are obliged to adopt to bring them into the stern-post in the case of single screw. I was in hopes that Mr. Scott Russell would have alluded to it when he was addressing us just now, because he could have explained it in a way that I will not attempt to do, but I am sure there is a great deal in it. As far as regards the records of revolutions, I think it only due to say I think few have attended more admiralty trials than I have, and I am sure the revolutions were always most accurately recorded. In the olden time, when watches were taken out of the pockets and used, somebody would begin one when he ought to have started naught, and in that way got quite wrong; but at the present time the revolutions are to be fully relied on.

Mr. J. A. **Thornycroft** (member). I should like to make a few remarks about the *Iris*, and to say that as far as the probable speed of the *Iris* mentioned by Mr. Wright is concerned, I quite concur with him, and consider that the fault in that vessel is due entirely to the screw. It somewhat takes me aback that we are not in possession of information that should enable us to determine the proper size of the propeller, so that it might have been designed at once for the vessel. I now beg to mention Mr. Froude's paper, to which I attach a great deal of importance, and I think the manner in which this subject has been dealt with offers great promise that we shall now have a method determined by which screws may be designed for vessels so as to fulfill the conditions thoroughly, and give the best results at once. I have made very many experiments with propellers myself, but I feel that now the ground is opened quite differently to me to what it has been previously. As to the angle of maximum efficiency which Mr. Froude mentions, it seems strange that we should find, after so many experiments, that it is a definite angle, whereas it was the previous impression of many people that that angle should be varied to suit a particular vessel. With regard to what has been said about the following wave in vessels, and the propeller being placed on that wave, the fact seems to have been neglected that that wave should follow the vessel. It is not by utilizing the motion of that wave and the propeller that the best result has been got, but by using the wave to support and balance the forward wave and its resistance to the vessel. The diagram on the wall showing the speed and horse-power of the *Iris* with the two-bladed pro-

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	Avni Illah.	*Muini Zaffer.*
*Area of immersed midship-section of vessel at measured mile trial in square feet	471.8	405
Total disk area of propeller or propellers in square feet	176.7	226
Proportion midship-section to disk area	2.67	1.79

peller, shows very clearly the great speed which is to be anticipated in the *Iris*.

Mr. E. J. **Reed**. I just wish to remove from the mind of Mr. Wright any idea that I spoke of the failure of the *Iris* as reflecting either on the designer of the ship or of the engines. I thought I took very great pains to avoid doing that; I tried to explain that I referred solely to the result. I wish to say that I quite agree with Mr. Wright in the hope that by adopting the modified plan he proposes, he will obtain the speed that was intended. I tried to avoid casting a reflection upon anybody connected with the construction of the vessel.

The **President**. Gentlemen, as Sir James Ramsden is not here, Mr. Woolley will give some explanation of this model of a yacht which is upon the table, and which I may say is a very interesting one.

Mr. A. **Sedgwick Woolley**. (secretary). The model is of a yacht that was built five years ago by Sir James Ramsden, in which the experiment was tried of putting a small screw in front of a larger one. He drives that by a small engine quite separated from the large one, and he finds that when the small screw is driven at twice the number of revolutions of the larger one, he obtains an increase of 2 knots an hour—the difference between 8 knots and 10 knots.

Mr. **William Henry White** (member of council). With reference to the case of the *Iris*, I should like to make a few additional remarks. In the paper itself I hope the case has been stated better than it was in my hurried *resumé* of the paper. These are the facts: If the *Iris* had been a single-screw ship, to steam 15¾ knots, with a two-bladed screw, such as is fitted in the *Volage*, she would have required over 4,500 horse-power, this estimate being based upon the actual performance of the *Volage* and what we know of the average ratio of effective to indicated horse-power. As a twin-screw ship, with the defective two-bladed screws (for they are so) which were improvised for the trials, she obtained that speed with less than 4,400 horse-power, which fact seems to show that the twin screws, even working under those disadvantages, with engines developing somewhat more than half their full power, were doing better than a single screw, driven by engines developing their full power, would have done in the same ship. Mr. Wright has dealt with the question of revolutions. I need not say a word about that point. As regards the use of the four blades adopted originally in the *Iris*, I desire to make one other statement. The four blades were adopted there originally because they had done well in other ships, and were known to reduce vibration, and to give a more even action. Even now I think I am right in saying that there is no final decision to use two-bladed screws in the *Iris*. The question is one for experiment, and it may be that three-bladed screws of small diameter and altered pitch would give as excellent results as could be obtained from two-bladed screws. As to the question of diameter, Mr. Thornycroft remarked how little we know about it, and I may add that in the

Iris the screws were made with an enormous disk area in proportion to their power. There is no doubt we have erred on the side of giving too great an area, but this proportion of disk area to engine power has always been done by a sort of rule of thumb. It must be always remembered that in single-screw ships of war there has been a sort of axiom that, with regard to the diameter of the screws, the larger it could be made the better. These facts must be considered in relation to the actual trials and performances of the ship. I do not think I need say anything more about the *Iris*. I will now come to Mr. Merrifield's remarks. I am quite at a loss to follow his objection to my statement. Merchant ships are, I am quite aware, of deeper draught in relation to their breadth, but they are also ships of very different proportions, and having very much less resistance than ships of war. The consequence is, as I have pointed out in the paper, that merchant ships with twin screws could use very small screws as compared with those we are compelled to use in ships of war, and these small twin screws in deep-draught merchant ships ought, to my thinking, to perform better than the twin screw of proportionately large diameters compared with the draught which are necessarily fitted in our large ships of war. The thing that seemed to me worth looking into in that comparison was this: what would be the amount of shelter for the twin screws in these merchant ships of narrow beam? I took a great deal of trouble, or rather it was taken for me by a friend of mine, to answer this question, and found that in the Atlantic steamers twin screws 13 to 14 feet in diameter could do all the work required. There is surely good reason for some one to make this trial, and to see whether in the deep-draught merchant ships, with these small screws well down, an economy of power is not obtainable; and, especially, I should say that if the efficiency of twin screws for merchant ships is to be tried at all, let it be tried in a vessel on the Australian line, where a break-down in the great stretch across from the Cape to Australia would be a matter of very great importance. There is only one other thing I have to say. Deep draught is a thing which diminishes resistance, and would give the twin screw a good chance in merchant ships. I am extremely obliged for the reception which my paper has met with, and I hope it may cause further attention to be given to a subject which I think is of very great importance.

Mr. E. FROUDE. I think, in the absence of the writer of the second paper, it is not necessary to say anything in reply.

NOTE.—The following communication was received from Mr. G. B. Rennie (member of council) subsequent to the meetings.—ED.

LONDON, *April*, 1878.

To the Secretary, Institution of Naval Architects:

In Mr. W. H. White's paper on "The comparative efficiency of single and twin screw propellers in deep-draught ships," read at the April meeting, it is shown that certain of H. M. ships (especially the *Alexandra* and *Temeraire*) require less power to propel them at their deep draught than at their light draught; but he does not seem to assign any cause for this remarkable fact.* I therefore think it might be interesting to the Institution to have their attention called to certain experiments made by Mr. George Rennie in 1856 (and in which I assisted and witnessed) to ascertain the pressure exerted by a screw propeller at different depths of immersion with a constant number of revolutions, and inclose a copy of the drawing and results of experiments for printing in the transactions of the Institution, from which one would infer that the cause of the deeper draught ships requiring a smaller proportion of power is due to the greater efficiency of the propeller when deeper immersed. For example, the resistance of the propeller, as shown in the experiments, was 49 pounds when the water just covered the propeller.

It was increased to about *four* times with 6 inches of water over screw-propeller, and about eight times with 4 feet 6 inches over the propeller. The propeller's diameter was only 1 foot 9 inches, and of equal pitch, the revolutions being constant at all depths.

Taking the area of disk of propeller at 346.3 square inches, these pressures give about—

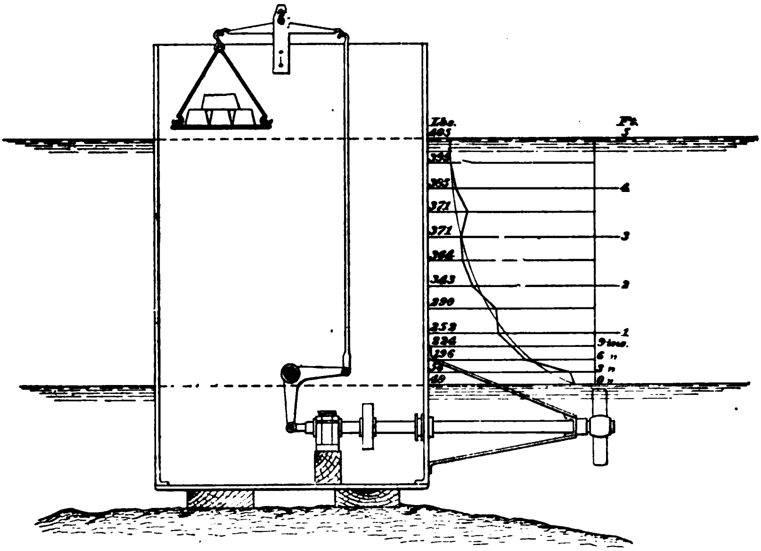
7 pounds per square inch with screw just immersed.
28 pounds per square inch with 6 inches over propeller.
56 pounds per square inch with 54 inches over propeller.

N. B.—This great extra resistance at the deeper draughts may account for the improved speed obtained in a yacht (a model of which was exhibited by Sir James Ramsden) by placing a small propeller near the keel.

The experiments were carried out as follows:

(1.) An iron tank (Fig. 1) was placed on the bed of the river in front of our wharf at Blackfriars, a shaft was passed through a stuffing box on one side of the tank, one end of which carried a screw propeller of 1 foot 9 inches diameter and equal pitch,

Fig. 1.



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*In Mr. White's paper, page 10, a reference will be found to the effect of deeper immersion in several ships on the performance of the screws.—Ed.

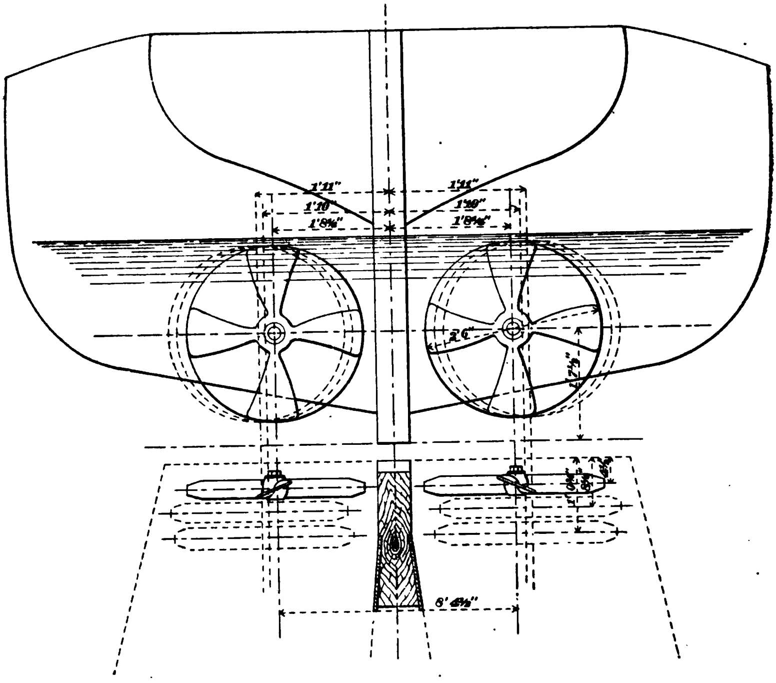
which worked in the open river, the propeller being of the ordinary two-bladed admiralty pattern, made of gun metal, and filed up bright to offer little frictional resistance. The other end of the shaft passed into the tank, in which also was the arrangement for taking the thrust by means of a right-angle lever connected with a balanced lever. The actual thrust was easily ascertained by means of weights in the balanced scales. A rigger was fixed about half way between the stuffing box and thrust, from which a leather belt was connected to the factory shafting and engine, so that a constant speed of 558 revolutions per minute could be maintained at all depths and resistance of propeller.

(2.) Mr. White calls attention in his paper to the effect of the position of the screw propeller. I may mention that I made certain comparisons of speed and power in some steam launches by placing the propeller nearer the stern-post. I found, taking the same power of engine, that when the center of propeller was only—

4¼ inches forward of stern-post a mean speed of 8.117 knots was obtained.
8¼ inches forward of stern-post a mean speed of 8.003 knots was obtained.
12¼ inches forward of stern-post a mean speed of 7.642 knots was obtained.

The drawing (Fig. 2) shows the relative positions.

Fig. 2.



Mr. White suggests whether the screw propellers can be protected. I may mention that I fitted a steam launch of 36 feet length with a protective casing round the propeller and tried the speed with and without the casing, and it did not appear to me that the speed was affected by its adoption; but that whereas the propeller "ran away" or raced with any slight swell due to a passing steamer, the same propeller when encased was not so affected.

Mr. R. Griffiths has been making many experiments on a small scale within these last few years, with a view to ascertain the effect of casing the propeller; but the experiments made by Mr. G. Rennie in 1867 showed that the thrust of the propeller when working in a conical tube was slightly decreased.

I am yours,

GEORGE B. RENNIE.

ON THE USE OF STEEL FOR MARINE BOILERS, AND SOME RECENT IMPROVEMENTS IN THEIR CONSTRUCTION.

By W. Parker, Esq., *Chief Engineer Surveyor of Lloyd's Register, Member.*

[Read at the nineteenth session of the Institution of Naval Architects, April 12, 1878; John Scott Russell, Esq., F. R. S., vice-president, in the chair.]

During the last few years several important improvements have been made in the details of marine engines and boilers by which the wear and tear has been lessened and the durability increased. The pressures meanwhile have been slowly but steadily advancing from 60 to 75 pounds, now a common pressure, and in a few instances 90 pounds has been reached. These gradual increases have also, I am assured by those engaged in working the steamers, been attended with a marked economy. But up to the present time all attempts that have been made with new forms of boilers, with the view of obtaining still greater pressures, have uniformly been signal failures, and a vast amount of money has been swallowed up in extensive experiments bearing on this point. To prove this I need only mention the names of the steamers *Marc Antony*, *Fairy Dell*, *Malta*, *Montana*, *Dakota*, and *Howard*, the boilers of which were constructed to work at a pressure of not less than 130 pounds per square inch. The recent adoption of mild steel for marine boiler making, however, will enable the present form of boiler to be made of greater strength, and gives promise of further advancement in the direction of higher pressures; and if experience is satisfactory as regards the comparative durability of steel and iron boilers it may not unreasonably be expected that this material will become almost universally used for their construction.

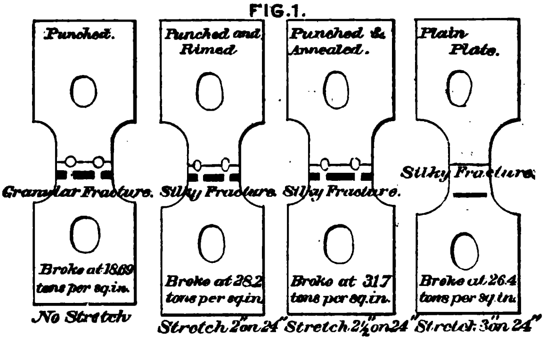
Some months ago the employment of steel for such purposes was ventured on by only one or two firms, but it is now coming into more general use; and at the present time there are about twenty-seven marine boilers being constructed of this material, under the inspection of the engineer surveyors to Lloyd's Register, with reductions in the scantlings of the shells and stays to the extent of about 25 per cent. In the first few sets of these boilers it was deemed advisable to submit the material to some severe tests. Pieces taken from the shell and front plates were subjected to tensile tests, and the mean of the results of over 80 tests gave a tensile strength of 27.5 tons per square inch, while the extreme variations of strength in the different plates, manufactured by different companies, and on different processes, was not more than 4 tons per square inch. Shearings of every plate used in the construction of the furnaces and combustion chambers were subjected to a tempering test; they were made red hot, quenched in water and bent cold almost double, with satisfactory results in each instance. In order to prove that the material does not suffer from the effects of repeated heating and cooling, I had two pieces of plate made hot and quenched in water repeatedly—one 50 times and the second one 100 times, but without any apparent injurious effect upon the temper of the material, the specimens so treated bending almost double without fracture, as may be seen from the specimen produced; further, one of these pieces has been tested, and its tensile strength has been found to be fully equal to the original plate. Specimens of the longitudinal riveted seams were also pulled asunder and their actual strength ascertained. Boxes constructed to fairly represent the back-water spaces of the boilers were made of steel and iron and tested by hydraulic pressure to destruction.

These experiments, which were not confined to steel manufactured on any particular process, and the results of which approximated so closely to each other, afforded pretty conclusive evidence of the fact that the improvements recently effected in the mode of manufacturing mild steel have insured the production of a material which, by its uniformity of tensile strength and ductility, &c., is eminently suited for all the purposes of marine boiler-making; and the satisfactory nature of these tests and the information gained thereby have enabled us to dispense with many of them in subsequent cases, the actual tests now required being much less stringent.

While this steel possesses such excellent qualities, however, it has been shown on several occasions that some of its essential qualities are liable to be injured to a much greater comparative extent than in iron by the operations usually occurring in boiler-making. For example, a great loss arises from punching when special treatment is not subsequently employed; it has been shown that this injury is to some extent proportional to the thickness of the plates and varies with the relative sizes of punch and die. In three series of experiments I lately conducted on steel boiler plates, the loss from this cause in some instances rose in ¾-inch plates as high as one-third of their ultimate strength, and in this respect these experiments agree with others previously made on plates of a similar thickness. The loss in thinner plates was found to be for ⅜-inch plates 18 per cent. ,for ½-inch plates 26 per cent., and for ⅝-inch plates 33 per cent. In order to arrive as nearly as possible at a practical result these specimens were prepared so as to represent the pitch and diameter of rivet-holes occurring in practice.

Four of these specimens which were cut from the same plate are

FIG. 1.



shown on Fig. 1, from which it will be observed that the plain piece broke at a strain of 26.4 tons per square inch with a fine silky fracture; the punched piece broke at 18.69 tons per square inch with no appreciable elongation and with an uniformly crystalline fracture; the specimen that had been rimed out after punching broke at 28.2 tons per square inch with an elongation of 2 inches in 24; while the piece that had been annealed after punching stood 31.71 tons per square inch and stretched considerably. The results of the tests on the other specimens were practically identical. The diminution in the strength caused by punching was thus 28.7 per cent. in comparison with the plain plate, while it is considerably more if we compare it with the rimed or annealed plate. The drilled specimens in each series stood as much as the solid plate, and in one or two instances there was even a perceptible gain in the strength of the drilled specimen.

The effect of punching in these specimens was not only to cause them to break at a less tensile strain, but it also prevented the specimens from stretching, causing them, in fact to break with a crystalline fracture. This at first sight would seem to indicate that the quality of the material had been deteriorated from hole to hole. It is not so, however, as these experiments show, and in this they confirm results previously obtained with material of a similar description. They clearly prove that the injury is confined to a zone of metal round the hole less than ⅛ of an inch thick; for when ⅞-inch holes were punched and then rimed out to 1⅛-inch diameter, the material stood as high a tensile strain as the plain specimen. It has been shown by careful experiments that in the process of punching steel plates the temper of the material becomes altered for a short distance round the hole, the metal becoming harder and less capable of stretching. Owing to its hardness and inability to stretch, this part round the holes when the piece is under tension takes a higher proportion of the strain than the other more yielding parts, and hence it reaches the breaking strain sooner. When the part round the hole gives way its strain is transferred to the other parts, and at first the extra strain falls suddenly on the adjoining parts, and this intense local strain apparently sets up a tearing action across the whole section of the plate, causing it to break at a low strain without stretch and with a crystalline appearance. As the loss from punching is so much greater in steel than in iron plates above a certain thickness, it is desirable to reduce this loss so far as may be practicable. Punching the holes small and then riming them, or drilling the holes entirely, or annealing the plates after ordinary punching, may be resorted to for maintaining the strength of the material in the structure. It also occurred to me that the hot rivet cooling in the punched hole might have the effect of annealing the disturbed part round the hole and restoring it to its normal condition, and in order to ascertain this I had a series of experiments made with steel plates $\frac{3}{4}$-inch and $\frac{7}{16}$-inch thick. The results of these are given in Table A, from which it will be seen that the cooling of the rivet did not affect the $\frac{3}{4}$-inch plates, whilst the strength of the $\frac{7}{16}$-inch plates was increased 1.8 tons per square inch.

Another operation which injures steel to a greater comparative degree than iron is flanging, and it is necessary in order to overcome the effects of this process to have the plates that are flanged subsequently annealed. If this precaution be not taken it will probably be found that after the work is completed the plates will crack, and this circumstance has given rise to many surmises as to the cause of the apparently "mysterious" fractures that have been found in some steel plates. In conducting the operation of flanging with ordinary boiler-maker's plant it becomes necessary to heat the plates locally, the effect of which is to induce in the vicinity of the heated parts a state of initial stress which in steel is much more severe than in iron. The annealing is sufficient, however, to set at rest all the disturbed particles, to relieve these strains, and thus restore the material to its normal condition.

It would thus seem that greater care and attention *must* be exercised in the manipulation of this material than with iron, and that while it is possible with careful treatment and proper appliances to practically maintain the original strength of the plates in the structure, the difference between the two materials and the consequent different manipulation required in each case must be realized, not only by the manager, but by the men who have to work the metal, for steel is much more liable than iron to be injured by the heavy usage to which materials are subjected in the ordinary boiler-yard. This, it seems to me, is a point upon which we cannot be too emphatic. When it happens that in consequence of improper or careless manipulations unsatisfactory and unlooked-for results are obtained, and preconceived notions are somewhat rudely shaken, it not unusually occurs that in trying to unravel the cause of the mishap attention is almost wholly confined to the composition of the metal, and little or no heed is paid to the manner of working it. Conclusions arrived at under such circumstances, being based upon a misconception of the actual facts of the case, might tend to throw unjust suspicion upon steel, and greatly hinder its more general introduction for marine boiler-making, and it is, therefore, incumbent on us to discriminate in such cases as far as may be possible between what is owing to the quality of the material and what is simply the *of a hot rivet in an*

ture.

large head; crystalline acture.

large head; crystalline racture.

arge head; silky fracture.

arge head: crystalline acture.

natural consequences of an absence of a due appreciation of the peculiar properties of mild steel.*

With regard to steel rivets, I may state that in all the boilers to which reference has been made at the beginning of this paper the rivets used are steel. It appears that if care is taken to heat them uniformly and not to overheat them, they may be used with as much reliability as iron rivets (I refer to rivets up to 1⅛ inch diameter). The experiments made on the riveted seams of these boilers have, however, disclosed the fact that the bar-steel at present supplied for rivets is not of so uniform a character as the plates of the same material; for while in one instance the rivets sheared at 27 tons per square inch, in another joint similarly proportioned the shearing strength was found to be not more than 19 tons. This wide discrepancy in the quality of material supplied from the same manufactory, and which had been thought to be of a uniform character, is somewhat startling, and demands a full explanation. The matter is one which primarily concerns the steel manufacturers, and it is now engaging the attention of some of the companies. When these investigations have been concluded we shall no doubt be in a position to place as much reliance on the shearing strength of rivet steel as we now do on the tensile strength of steel plates; but in the mean time until we are reassured on this head it is desirable to ascertain by actual test the quality of the rivets employed, and this is done in each instance in the boilers now in course of construction.

The experiments on riveted seams have also shown that if double-riveted joints are made of steel with the same ratio of bearing surface to area of plate as is usual with iron, there is a likelihood of the plate giving way by crippling or upsetting behind the rivets, causing the holes to stretch oval and the joints to open at a very low strain compared with the ultimate strength of the material. So that a pressure of little more than twice that at which the boiler is designed to work, or, say, one-third of the bursting strength, would cause the boiler to leak. It will therefore be found necessary in proportioning steel joints to allow a greater amount of bearing area than would be done in iron joints.

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* I may here give a practical illustration bearing on this point which came under my observation this morning. In a letter from one of our engineer surveyors he reports the unexpected fracture of two furnace plates in steel boilers being constructed under his inspection. It appears that the furnaces were not exactly the same diameter as the front plates in which they were intended to be riveted, and that they had been drawn and hammered to make them come in. The riveting of the front seam of the furnaces was little more than half finished, and was being proceeded with when the plates in both cases cracked from the rivet holes, for a distance of about 8½ inches, at a considerable distance from the part then being riveted. The material was not of a brittle nature. A specimen tested showed a tensile strength of 27.6 tons per square inch, and the tempering and bending tests were also satisfactory, cuttings from these two plates having doubled to $\frac{7}{16}$ without any appearance of fracture. This result proves that although such treatment may to a certain extent be admissible in the case of iron boilers, it must be avoided in the case of steel.

The only important point in connection with this subject regarding which we have scarcely any reliable data, is the question of the comparative corrosion in steel and iron marine boilers, and although the analytical experiments that are now going forward on a small scale may furnish some interesting particulars it seems probable that we shall have to wait for the disclosures of actual experience. On a recent occasion, when I had an opportunity of examining the boilers of two vessels fitted with steel and iron boilers working under the same conditions, the steel boilers had apparently suffered more than the iron. In the steel boiler ten years old it was observed that the corrosion had been most capricious in its action, and the effects on contiguous plates which were to all appearance subject to precisely similar conditions were very different. The corrosion also seemed to be very severe in the narrow water spaces in which, from the defective circulation, the water would at times be driven from the surface of the plates. By the courtesy of the superintendent engineer of one of these steamers I was enabled to have a specimen cut from two plates which showed vastly different results so far as regards corrosion; but upon being analyzed they were found to be pretty nearly alike in their constituent elements, the slight variation not being more than would probably occur in plates from the same ingot. The percentage of carbon was very much higher than is usual in plates manufactured at the present time, and judging from the analysis the original strength of the material could not have been much short of 40 tons per square inch. Table B gives the analysis referred to, which was repeated by three different chemists, and showed practically the same result:

TABLE B.—*Analysis of two plates contiguous to each other in a steel boiler working under identical conditions, and which showed very different results as regards corrosion.*

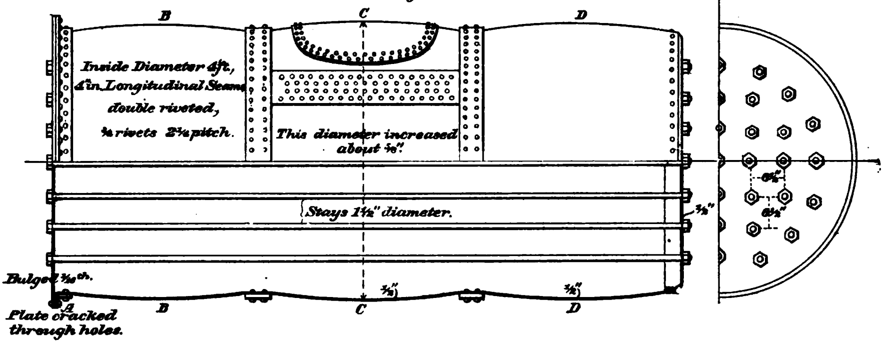
Percentage composition.	This plate badly corroded. No. 1.	This plate in good condition. No. 2.
Combined carbon	0.41	0.40
Graphite	None.	None.
Silicon	Trace.	Trace.
Manganese	0.25	0.25
Sulphur	0.05	0.06
Phosphorus	0.02	0.02
Iron (by diff.)	99.27	99.27
	100.00	100.00

It is only right to remind you, however, that at the time when the boilers in question were made the manufacture of mild steel had not advanced to such a state of perfection as has since been reached, and it would therefore be unsafe to accept these results as a criterion of the effects of corrosion on the steel of the present day.

With regard to the corrosion of shells I think it is to be regretted that the longitudinal seams in shells are not made with double butt-straps, as, if this were done, and the inner strap and rivet heads were made of extra dimensions, a uniform corrosion over the entire shell to the extent of, say, about 30 per cent. would still leave the structure at its original strength, viz, that due to the strength of the longitudinal seam.

Through the kindness of Mr. W. Dean, locomotive superintendent of the Great Western Railway Company, I lately had an opportunity of witnessing at Swindon an attempt to burst an experimental steel boiler, representing the barrel of a locomotive, as shown on Fig. 2. The internal diameter of this boiler was 4 feet 4 inches; it was made of ½-inch plates double riveted with ¾-inch rivets, 2¼-inch pitch, the joints being double butt-strapped, and the workmanship extremely good. At a pressure of 728 pounds per square inch the end plate cracked between the rivet holes for a considerable distance round the plate marked A, and at 795 pounds the increase on the original diameter of the barrel was at the parts marked B, C, and D ⅝ of an inch, whilst at the bands it was only $\frac{1}{16}$ inch. A hole in the top plate, into which was screwed the connection to the pressure gauge, was stretched about $\frac{1}{16}$ inch oval, and the leakage at this hole, at the crack and at the joints, was so great as to completely overpower the pumps. Table C gives the detailed results of this experiment, which seems to indicate that, in consequence of the great stretch of the material, the leakage at the joints in steel boilers, even although double butt-strapped, would give warning of the excess of pressure long before the bursting point was reached.

Fig 2.



At present it is scarcely practicable to rivet together with ordinary appliances plates of a greater thickness than, say, 1¼ inches, but it seems not improbable that we may before long be able to dispense entirely with longitudinal riveting, and thus minimize the difficulty now experienced in working heavy plates. The idea has now been revived of rolling boiler shells in broad solid rings, and I am assured by several of the steel makers in this country that it is quite practicable to produce by this method cylinders of from 9 to 11 feet in diameter. A step has already been taken in this direction by an eminent steel manufacturer, who is now rolling solid cylinders of large diameter, which, when attached together, it is intended to work with compressed air at a pressure of 1,000 pounds per square inch. Fig. 3 is a vessel constructed in this manner, and Fig 4 illustrates the same principle proposed to be carried out in the case of an ordinary marine boiler.

TABLE C.—*Test of experimental steel boiler.*

DESCRIPTION OF BOILER.—Length inside 10 feet 6 inches, diameter inside 4 feet 4 inches; plates $\frac{1}{2}$ inch thick, band 5 inches wide, $\frac{9}{16}$ inch thick, two rows of $\frac{3}{4}$-inch rivets; thirty-seven longitudinal stays (steel) 1$\frac{1}{2}$ inches diameter, with 1$\frac{3}{4}$-inch ends, and nuts; angle iron at flat ends, Low Moor iron, 3 by 3 inches by $\frac{1}{2}$ inch full, $\frac{3}{4}$ inch thick; flat end plates (steel) $\frac{1}{2}$ inch thick.

Order of test.	Pressure per square inch in pounds.	Increase in circumference at bands in inches.	Increase in diameter.	Bulging of end plates (flat).	Bulging of end plates (flanged).	Result.	Remarks.
First	252	Nil.	Nil.	Nil.	Nil.	Tight.	
	336	$\frac{1}{32}$	Not perceptible.	do.	do.	One longitudinal stay leaking, 3 rows from bottom.	Stopped test and took off nuts of stays to caulk.
						One do.; do. at top.	
Second	336	$\frac{1}{64}$	do.	do.	Nil.	Tight.	
	448	$\frac{1}{16}$	do.	$\frac{1}{8}$	do.	Do.	
	560	$\frac{1}{16}$	do.	$\frac{1}{8}$	do.	Leak at brass plug.	
	616	$\frac{1}{16}$	do.	$\frac{1}{8}$	do.	Heavy leak at brass plug, rivets in band, and angle-iron ring.	Stopped test, caulked band nearest flat end of boiler and tightened brass plug.
Third	616	Not gauged.		Not gauged.		Tight.	
Fourth	616	$\frac{1}{16}$	Not gauged.	$\frac{1}{8}$	Nil.		
	694	....	do.	$\frac{1}{8}$	do.		
	728	$\frac{1}{8}$ full.	do.	$\frac{3}{16}$	do.	Band split transversely 1 inch between rivets, brass plug leaked badly owing to hole in plate, elongating flat plate cracked, for 2 feet 6 inches length between rivet holes.	Permanent set at flat end $\frac{1}{16}$ inch.
Fifth	336	....	....	$\frac{3}{16}$	Nil.	Slight leak at crack rivets; do. flat end.	
	588						
	700	Not gauged.		not gauged.		Sharp crack, flat plate broken between rivets, leaking badly.	
	784						
	795	$\frac{1}{8}$	$\frac{5}{8}$	$\frac{5}{16}$	....	Crack in flat plate leaking badly, rivets leaking, hole in plate with brass plug for pressure gauge drawn oval and leaking badly, so much so as to render it impossible to increase the pressure.	

FIG. 3.

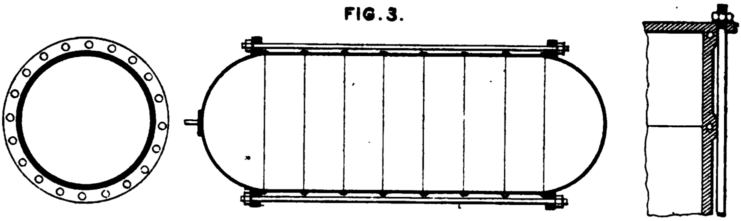
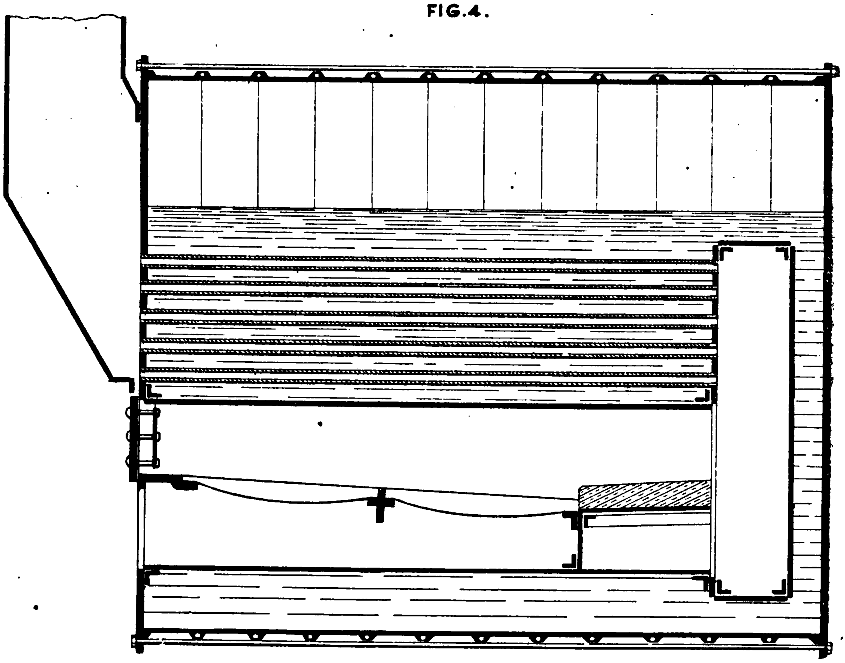
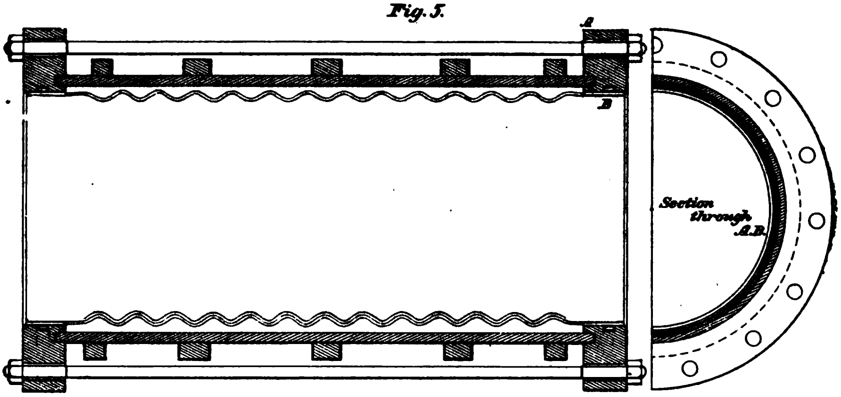


FIG. 4.



But while we are thus increasing the strength of shells the one weak point in the boiler is the furnace; for although steel furnaces will undoubtedly be stronger than iron ones of the same dimensions, yet the transmission of heat demands that the thickness of the furnace shell shall not be materially increased, and the stiffening rings necessitating riveting, &c., which are found in high-pressure boilers invariably occasion trouble at sea. It is therefore gratifying to mention a recent improvement which bids fair to fulfill all the requirements of the case, by supplying the necessary strength without increasing the thickness of the plates or having recourse to objectionable strengthening rings. This consists of a corrugated form of flue patented by Mr. Sampson Fox, of the Leeds Forge Company, to whose courtesy I am indebted for Fig. 5 showing this flue, together with the apparatus employed for testing it. The strength of this description of flue seems to be that due to

Fig. 5.



the direct crushing strength of the material, and the experiments recently conducted to ascertain its collapsing strength, in comparison with the ordinary plain flue, showed that the corrugated iron furnace had at least twice as much strength as the plain one; so that the working pressure could with safety be doubled so far as the furnace is concerned. Of course, if this form of furnace were rolled in steel the increased stiffness of the material would produce a corresponding increase of strength. From the peculiar form of the flue it also possesses a more extensive and therefore more efficient heating surface than that provided by the plain furnace.

Now as we have a material that gives us a boiler about 30 per cent. stronger than an iron boiler of the same scantlings, and as it seems possible that we may be able in the immediate future to dispense entirely with longitudinal riveted seams by having the shells rolled, and as there has also been a furnace introduced which can be worked at twice the pressure of the ordinary plain flue, it does appear to me that we have succeeded in a great measure in removing the old conditions that have militated against much higher pressures being obtained, and that we appear to be now in a position to make a fresh departure in the direction of still greater pressures. If the improvements which I have indicated prove, as I have little doubt they will prove, successful, we shall have gained an advantage represented in the aggregate by an increase of about 80 or 90 per cent. of the working pressure. In other words, we shall be able to work the present form of boiler at 160 or 170 pounds per square inch; and, although the resultant economy will not be so great as that which attended the increase at one step from 30 to 60 pounds, we may confidently anticipate that it will be sufficient to give a great impetus to steam navigation, advancement in which has lately been so much retarded by the high consumption of fuel.

DISCUSSION.

The **Chairman** (Mr. J. Scott Russell, F. R. S., vice-president). I am sure that we must have all heard this paper with very great pleasure. In the first place, the information which it gives us is exactly the information we want; and, in the second place, it is conveyed to us by a most competent gentleman, and conveyed to us as the result of very serious experiment. For my part I am very anxious that steel should be introduced, because of the great advantages we should derive from it. I am very anxious for the same reason that steel should not be prematurely introduced, or in any doubtful or ambiguous manner, and to show us how to avoid that evil is one of the great values of this paper. I am very glad that Mr. Parker has touched on one point, which I will call your particular attention to, namely, the point of the internal furnace or tube, because I need not say to you one of the great defects of steel would be this, that while a much thinner plate would stand the external force, a thin plate would be more liable to crumble from the external force, and that would be an evil which in its simple form could not be got over. I consider it a valuable point in this paper, to which I beg your attention in the discussion of it, that this almost unavoidable evil is most elegantly got over by an invention, which really is the carrying out, in a much more refined form, of an old invention of our friend Professor Fairbairn, who invented the putting a ring all round, so as to give it depth without too much thickness. I hope you will give a thorough discussion to this paper, because I think we are at a critical moment with regard to the introduction of steel for boilers; and I think also, at this moment, it is of very great consequence to steam navigation thoroughly to study how you are to get the highest pressure with the greatest safety and the greatest efficiency. There are many gentlemen here who have given the subject of boilers and steel their careful study, who I am sure will be able to speak upon this subject, and we shall be all grateful for any additions to this most valuable paper.

Mr. A. C. **Kirk** (member). Mr. Chairman and gentlemen, as I had the honor to read a paper at the Glasgow meeting on riveting and steel plates, and as I have had some little experience in their use, will you permit me to make a few remarks on Mr. Parker's paper? I think Mr. Parker in this paper has done us very excellent service. He has brought together the experience up to this time in the use of steel, and being a new subject it is of great importance to us all that that experience should be brought up to date. There has been immense progress made in the manufacture of steel, and I am also happy to say there has been great progress made in the method of treating it by those who use it. There need now be no fear as to the manufacture of wholesome steel for the purpose of riveting or plating. Steel may be punched or drilled, but if it is punched it must be subjected to treatment which, though beneficial in the case of iron plates, is I think I may say absolutely essential in the case of steel plates. As to the cause or theory of the great diminution of strength when steel plates are punched compared with iron ones, it is perhaps premature to say much, still I think it may be said to be entirely due to the punch producing a ring of extremely compressed material round the rivet hole, which, expanding in diameter under the pressure of the punch, acts like a wedge, and brings the whole section of the plate into an extreme state of tension. I think that is proved by the fact that where you drill out a thickness of about one-eighth of an inch, which seems to be the extent to which this metal extends, then the plate is relieved from the bursting stress which is put on its rivet space, and the rest of the metal yields pretty nearly its full strength. In iron that does not happen to the same extent, which is mainly due to the looser texture of iron. Wrought iron is a looser sort of material. When put together by the process of welding it is intimately mixed with little particles of cinder, and it does not seem as if strains were propagated through steel in the same way as they are propagated through the looser material that iron is. I do not know that I make myself quite clear in the matter, but I will endeavor to describe to the Institution the experiments which I made six months ago. Take the holes in the plate, for instance, which we tested. Unless we had a much larger section of plate from the pin-hole to the end than you require in an iron plate, the steel plate split under the pressure. You could in them see quite well inside the hole that the pin of the testing machine had compressed the metal, thickened it up, and as it were inserted a wedge in the plate. When fracture occurred in this way, the fracture propagated through the plate was invariably crystalline, although the same plate when broken asunder in the testing machine exhibited a silky fracture. I am not prepared with any theory to explain that. But the same thing showed itself when the punched holes were torn asunder, and when the compressed metal inside the rivet hole had not been previously removed; the silky fracture was lost very much, and a sort of crystalline fracture set up.

There is one other point—the corrosion of steel plates—and there I quite agree with Mr. Parker, that we must depend more upon the result of actual experience than upon laboratory experiments made with definite chemicals, because as to boilers I think chemists do not know much more than we do what are the special corrosive agents, and it is rather uncertain work to reduce it from the steel used some years ago, because most of it was puddled steel, in which (and Dr. Siemens will correct me if I am wrong) no manganese was used; and if an excess of manganese is the cause of abnormal corrosion, it is quite possible the results drawn from experiments with puddled steel might not be fairly applicable to the steel of this day. One thing I noticed in the building yard in working steel was this: That while most of the steel welded quite freely, if the smith lost his first heat and tried a second heat, it was extremely difficult to get it to weld. I asked some chemical friends of mine what they thought could be the cause of this, and the suggestion made (as to which I am quite open to correction, as it is strictly theory) was, that there might be oxide of manganese formed from the excess of manganese in the steel. However, there is no doubt that as the quality of steel is improved it is approximated more and more closely to pure iron. I feel quite confident of that. Yesterday I recounted to the meeting some experiments of welding together slabs composed of cuttings partly of steel and entirely of steel. All of these welded perfectly. I do not know what the percentage of manganese was in them, but they were remarkably good plates, and probably it was low. The best steel at the present day may be considered as very pure and perfect iron, differing chiefly from iron in its mechanical texture in being perfectly close and homogeneous, and having no open spaces or foreign bodies intermixed with it. I am very glad also that Mr. Parker has brought before the notice of this Institution this arrangement of corrugated flues. Of course there is no doubt that the flues of marine boilers have been, at very high pressures, the most unmanageable part of the arrangement. We have adopted different kinds of stiffening rings, some of which have done very well on the whole; that is to say, rings detached from the furnace, and attached only to the furnace by a succession of studs exactly like the bridges across the top of a locomotive fire-box; these extended around and did not interfere with the access of the water to the furnace plate, and certainly gave a great support. But there is no doubt that this corrugated flue is an improvement and a very elegant improvement. I do not really attach so much importance to the increase of heating surface; I think that has been made too much of; but the flue is such a good one that it does not require those adventitious excellencies to be made so much of as they have been. The fact is, the flue is a strong flue and a light flue, so strong that it will enable higher pressures to be carried. Mr. Parker further told us that he had made a number of experiments upon the staying of flat surfaces. That is such an important point that I hope at another meeting Mr. Parker or somebody else will be able to lay before us a complete set of experiments bearing upon it. I do not know whether Mr. Parker's experiments have gone so far as to enable him to do so yet; I suppose they have not, or he would have given us the benefit of his experience. That is a point on which perhaps we know less than upon any matter in connection with the staying of boilers—about the deflection of the squares between stays in flat plates. The point of interest is not so much to my mind the ultimate bursting strength of the surfaces, as the pressure at which deflection commences, because, with screwed stays fixed through the plate, as soon as deflection commences the attachment is nearly gone. The plate bends very much like an India rubber valve when you pull it by the center and opens off the screw. If it is riveted the small rivet will probably give way, and the plate is blown off the rivet-head at a pressure much below what it should be. If there is a nut behind to support it, it will not probably be blown off at such a low pressure, but the result is not much better, because the plate opens from the screw, the salt water gets access to it, and the thread is speedily destroyed, and leakage commences to such an extent that in a short time danger will result.

The CHAIRMAN. Do not you generally use both an internal nut and an external one, and also a washer of metal under each; and does not that make a perfect end to the stay?

Mr. A. C. KIRK. I am very glad that you brought up that, because perhaps it may tend to make the matter more clear. In the case of staying the end of the boiler, as we see here, we should most probably do as you have said, and it would be very good practice to do it. I was rather speaking of staying the flat sides of furnaces. Then there is another objection to putting nuts on stays, that if the temperature is very high, or if very flaming coal is used, these nuts almost always get overheated, and they then expand and ultimately get slack; and the only arrangement, as the experience of locomotive engineers has pretty well settled, is to screw a stay through and rivet it; but such a surface is practically done for when deflection of any sensible amount takes place. We are indebted to the gentlemen who have introduced the materials to us, and also to our friends at Lloyd's, who have completely broken through the traditional and artificial rules and proportions that some of our other friends in power have saddled us with—excessive proportions, excessive weights, and many other things that we need not here enter into, but which at all events amount to a very serious incumbrance, not only on the manufacture of engines, but on the remunerative and paying power of ships after the engines have been manufactured. I hope that the influence of this Institution will, in due course of time, be brought to bear further upon this subject, so that we may have relief from some of these oppressive regulations, and follow more in the wake of what this very responsible body of Lloyd's has pointed out. I hope that I have not occupied too much of your valuable time.

Mr. JAMES WRIGHT (vice-president). There is just one point in Mr. Parker's very excellent paper which leaves a little doubt in my mind. I quite agree with him in his opinion as regards the excellent quality of steel now to be obtained, and as to the facility with which it can be worked up into boilers. All that can be done now without fear of bad consequences. I think he said he had agreed to a reduction of thickness of 25 per cent. That is the difficulty I have. All of you are aware—and no doubt there are many members here who are connected with the Navy—that we have suffered very much from the corrosion of boilers; and the corrosion of steel is, I think, still an obscure subject. So far as our experience in the Navy goes it has not been fortunate. A number of years ago a quantity of steel boilers were made, not of such steel as we can obtain at the present day, but considerably stronger and harder. I believe the breaking strain was from 35 to 36 tons, and so brittle was it in some cases that it split across in punching. Still it did not resist corrosion at all well when used with salt water in the boilers. One case was very bad; it was a low-pressure box boiler working at 16 pounds to the inch, and wore out in about 2½ years. I should say that that boiler was made only ¼ inch thick instead of three-eighths, which it would have been made if iron had been used. Take a ½-inch plate, and then take 25 per cent. of thickness off that.

Mr. **William Parker** (member). The reduction of 25 per cent. is for cylindrical shells and stays only, not for flat plates.

Mr. J. **Wright**. That makes a difference; but still if the iron and steel shells are only of equal strength at first, and the rate of corrosion is equal, the steel shell will lose strength more rapidly than the iron one. I was going to say this also, that the boiler committee have been making a great many experiments on the subject of corrosion of steel, and the general conclusion which they come to was that it was rather more rapid than the corrosion of iron. The doubt in my mind is whether it would be wise to reduce to such an extent as Mr. Parker mentions the thickness of the plates. A short time ago the admiralty ordered some steel boilers for the *Iris* of the same quality of steel as the hull was made of. A beautiful material it was, but on account of the question of corrosion it was not thought desirable to reduce the thickness much. It would have been ¾-inch thick if it had been made of iron, and it was only reduced to eleven-sixteenths. That was simply from the fear that the corrosion might be more active than, or at least as active as, in iron.

Mr. W. **Parker**. Shell plates?

Mr. J. **Wright**. Cylindrical shell plates. On the other hand we have had for many years in the Navy a great many small boilers in steamboats and pinnaces, and I think they have lasted very well. They were made a ¼-inch to begin with, and five-sixteenths, and generally lasted about a commission. In that case they are generally used with fresh water or water nearly fresh. There is one other subject I should like to mention, and that is with regard to this corrugated flue. I understood that Mr. Parker was present at the experiments which were made, and I will leave him to tell us how much the flue extended lengthways under pressure. Boiler flues, as used generally, serve as stays to the boiler, and I fancy that a corrugated flue might have a certain amount of elasticity which would deprive it of some of its use as a stay. On the other hand the plain flue was, I believe, free at the ends, and that it collapsed under a pressure of 200 pounds. I think if it had been fixed at the ends it would have stood a greater pressure; but I think that the experiment did not give fair play to the plain flue, because if it did, we have been working with plain flues with rather too small a margin of safety.

The **Chairman**. I would merely call Mr. Wright's attention to the interesting fact, which he is acquainted with no doubt, that in the inside of boilers it often happens that a thin plate lasts longer than a thick one, the reason being that owing to the non-conduction in a certain degree of heat through the plate, the inside of a thick plate becomes more overheated than the corresponding side of a thin plate. That is on the other side, and may in some degree counterbalance the argument.

Mr. **JOHN R. RAVENHILL** (member of Council). The paper we heard yesterday read by Mr. Martell, followed as it has been to-night by this paper of Mr. Parker's, must satisfy everybody that the time has arrived when steel is to be used most extensively if not entirely both in the hulls and in the boilers of our commercial marine, and doubtless also in vessels for the Royal Navy. Those gentlemen have given the institution the results of very elaborate and valuable experiments, and we ought to appreciate the kindness of Lloyd's in giving us those results, and not only us but also the world at large. There was a time, and it is not so very far distant, when Lloyd's Register of Shipping was looked upon as old-fashioned. It was thought to be useless to go to them; but Mr. Parker certainly has eclipsed anything that has been done to my knowledge, because he has given us the results of information received up to the morning of the reading of his paper. He has certainly broken the rule of keeping the paper till the last moment, but I daresay the Council, under the circumstances, will be willing to condone the offense. He has alluded to experiments that he is making, I believe at the present time, if I gather rightly what has fallen from him, on flat surfaces, and he proposes to continue them. I take it that there are few things that would be more valuable or more interesting than a collection of experiments on stayed flat surfaces—not on the plates as in water tanks, but with the plates subject to various temperatures, representing, in point of fact, the flat surfaces of the combustion chambers in high-pressure boilers. The Americans, some years ago, did conduct a series of experiments, I believe, and they were published; but nothing much has been done in England since that date to my knowledge; and now that Lloyd's Register of Shipping have come to the fore and have gone thus far, I hope they will go still further and carefully carry out those experiments and have them accurately recorded. We want a great deal of information on this particular subject, and nobody knows better than Mr. Parker himself that the want I allude to is in every sense a real want. While he is conducting those experiments he will doubtless also learn how best he may fit the stays in plates so as to prove a point upon which there is a difference of opinion, and to which some allusion has been made. It will then be seen whether the best results will be obtained by stays being tapped into the plates with a simple nut or with a washer and a nut, or riveted heads, or in any other form which he may like to try during those experiments. He has brought, as I said before, those experiments up to the present time, and knowing myself how carefully they have been conducted in his hands, I feel there is nothing we can say on the information he has placed before us to-night except, perhaps, to ask him a few questions. There is a question about that corrugated flue which I should like him to give us his experience on. Some time back I was talking to him about that form of flue, and expressed a doubt as to whether difficulties might not arise unless the perfect form throughout could be maintained; and it occurred to me that practically some difficulties would be experienced in maintaining that circular form just at the outset of their use, and it was a matter which I thought out to be seriously looked to. I have no doubt he has done so, and can give us some information upon it, although he did not allude to it in his paper. Mr. Wright made some remarks as to the elongation of that form of plate, and I quite agree with every word Mr. Wright uttered. There is one point I should like to allude to in connection with steel boilers, and it is this—that there is such a vast difference in the management of boilers, in the every-day working of the boilers, between the vessels in our commercial marine and in vessels of the Royal Navy, that I think Mr. Wright is prudent in adhering to a greater thickness of plate than other gentlemen are inclined to use in the commercial marine. As we all know, Her Majesty's vessels leave England for long terms—perhaps for three years—and we know very well that if anything does go wrong with a vessel of the Royal Navy there are plenty of kind friends to publish it. With the commercial marine it is different, and a vast deal goes on there that nobody knows anything about, unless it is just those who are connected with the case and a few surveyors who may be fortunate enough to hear of it, and able to obtain the experience it gives them. I think if Mr. Parker can supplement his paper in the way I have stated he will increase its value —a value which we must all admit is very great.

Mr. **Boyd**. As a visitor here this evening, I have some reluctance, Mr. Chairman, in intruding myself upon the meeting, but I think I am in a position—as the author of the paper that has been read at a neighboring institution this morning—to make two or three remarks on the subject which has been alluded to by the two previous speakers, namely, the staying of flat surfaces. I may say, in explanation, that these experiments were conducted under the supervision of Mr. Parker and Lloyd's Registry, and they were conducted on the first boiler which was submitted for their approval. The experiment that was made was a very simple one, but the results shown are rather interesting. Two boxes were constructed representing as nearly as possible a piece of the combustion chamber of a marine boiler. I will take for illustration the end of the combustion chamber in Fig. 4 of Mr. Parker's paper. These boxes were about 3 feet square, and they each of them contained nine stays. I may say that the original object of the experiment was to exhibit the different degrees of buckling in iron plates of ½-inch thick and steel plates of seven sixteenths. That was the original idea with which the experiment was undertaken. The experiment was conducted with the stays riveted over in the usual way—that is to say, without nuts and without washers, or supports of any kind, the stays being simply screwed into the plates and riveted over in the usual way. It is a matter of interest, but the question at issue, as I understand, is not so much the relative buckling of iron and steel as the relative buckling with different kinds of support.

Mr. J. R. RAVENHILL. At different temperatures of the plate?

Mr. BOYD. Of course the different temperatures of the plate add an additional interest to the experiment. I am quite willing to admit that, but it is open also to the difficulty of carrying out the experiment. The difficulty of conducting an experiment and measuring accurately the amount of buckling of a steel or iron plate while that iron plate is subject to the intense heat of a furnace is naturally a difficulty that will, I think, occur to the minds of most here. I regret to say that that difference of temperature is not one which we were able to accomplish; but at a normal temperature the experiment was performed. The plates, as I say, were riveted over in the ordinary way, without nuts and without any additional support on the heads. Without going into details or wearying the meeting, I may say the buckling took place at 130 pounds in steel plates, and the box ultimately burst at 550 pounds. I I will not trouble the meeting with the result as to ordinary plates. The box I mentioned burst at the lip of the riveted stay, that is to say, the head of the rivet flew off, and we were unable to continue the pressure any longer. Having these boxes made, it occurred to us that it would be a matter of interest to try another set of boxes with nuts on the heads on the outside, only not on the inside, as is represented on the back of the boiler in the drawing. The buckling commenced in that case not at 130 pounds, but at 260 pounds, that is to say, the plate was stiffened by the nuts and washers to the extent of 100 per cent.

The CHAIRMAN. Were there washers on the nuts?

Mr. BOYD. Yes; the diameter of the washers was the same as the points of the nut.

Mr. RAVENHILL. What was the extreme diameter of the rivet head?

The CHAIRMAN. Do you remember the thickness of the plate and the diameter of the bolts?

Mr. BOYD. I can tell you those exactly. The thickness of the steel plate was seven sixteenths; the diameter of the stay bolt was 1⅜ inches; and then the diameter of the head when riveted over would be about 1⅞ inches.

Mr. WRIGHT. Would you propose an arrangement of nuts and washers for the inside of furnaces?

Mr. BOYD. I will give you the result of the experiments first. The plates, as I say, were fitted with washers. The buckling showed at 260 pounds, and the bursting strain rose from 550 pounds to 950 pounds, and the fact is that the plates never cracked at all. Before the plates could be cracked, the structure of which the box was made gave way; but in the case of the steel and iron plates the flat stays on which we were experimenting remained uncracked and uninjured in any way, but at 1,000 pounds the strain was so great that the box to which these flat plates were connected gave way, and the experiment was at an end.

The CHAIRMAN. What was the distance of the centers of the plates from each end?

Mr. BOYD. Nine inches. With reference to the question of fitting nuts inside the combustion chamber, I know that that is a difficulty. Probably every gentleman present is well aware, much better than I am, that there are many boilers so fitted; and provided the nuts are not too thick, and provided the boilers are kept clean, and the nuts properly looked after, I think there is no particular danger; and no doubt, as is shown by these experiments, to which I have already referred, you get an immense increase of strength; but if the nut is allowed, or in fact if the boiler generally is allowed, to become neglected, and a large amount of deposit is allowed to take place in the narrow space at the back of the nut, and the boiler is not kept clean, no doubt the plate is very apt to burn away, and you rely upon something which is unreliable; but it is a thing which is, I will not say, common in practice, but which exists in practice, that these flat surfaces should be fitted with these nuts, and I think that the immense amount of additional strength given to the flat surfaces by the fitting of these nuts certainly warrants us in taking into consideration whether they might not be advantageously used. I should like to ask Mr. Parker one question, and it bears upon this same point. In regard to Fox's furnace it has always appeared to me to have this element of danger in it. On the upper side of the furnace those corrugations would become the receptacles for deposit which would be liable to injure the plate, and then the flame striking the under side of the curve on the reverse side of which would lie this deposit, the plate would be very apt to burn away and to crack where the deposit was made. I should be glad to hear Mr. Parker's opinion upon that.

The CHAIRMAN. Is there not another point of view—that there might be rather more current downwards in that very groove, and so carry the deposit to the other side?

Mr. J. FORTESCUE FLANNERY (member). Mr. Chairman and gentlemen, this paper is a very interesting one, not only on account of the subject, but, I think, for a stronger reason. It shows what are Mr. Parker's views, and we all know that Mr. Parker, by reason of his official position, can do a great deal either to assist or delay any improvement in the mercantile marine; and it is very satisfactory to find he is not one whit behind Mr. Martell in his eagerness for progress, and, indeed, in his paper he has, perhaps, shown a tendency to go a little further forward than some other engineers who follow him. One remark fell from Mr. Kirk that I think hardly had the importance attached to it by the meeting which it deserved, and that is that the metal we are now using has very little claim to the title of steel, but is almost entirely pure iron. So lately as the latter end of 1875, it became my duty to report upon the desirability of fitting steamers with steam boilers. I had not the opportunities Mr. Parker and others have had of knowing what was being done throughout the country at the time, but I became acquainted with all the methods of manufacture, and the results obtained by the Bolton Iron and Steel Company. I also knew pretty thoroughly what was being done by Mr. Webb, at Crewe; and having this information I consulted with the manager of one of the first firms upon the Thames—one of the most experienced engineers in London—and we came to the conclusion that with the materials then in use it was not at all desirable to report in favor of the use of steel for boilers. That firm had already made one set of boilers of steel with very contradictory and irregular results. That was so lately as the latter end of 1875, and I mention that circumstance because I wish to give more point, if possible, to Mr. Kirk's remark that the material we are now using has no claim at all practically to the name of steel. That being so, we must entirely cut ourselves off from the experience that we have had with steel in boilers, because the material we are now using is so entirely different that the experience we have had is no criterion as to what the results may be which you may get from the material as at present supplied. I had the privilege of hearing a paper which was read by the last speaker at the Institution of Mechanical Engineers this morning, and I think he might, with great propriety and great advantage to the meeting, have dwelt even more upon the facts he gave in that very able paper. I studied it carefully, and it seems to me that four very important conclusions were arrived at by Mr. Boyd, not merely from his experiments but from his experience in manufacturing boilers—the very first pair of boilers of steel that went to sea. The conclusions he arrives at are that with the present system and facilities for manufacture we can obtain plates of almost perfect uniformity with the restrictions as to test which are being insisted upon by Lloyd's. There is no injury to these plates by drilling, and the only injury that is produced by punching can be perfectly annihilated by subsequent annealing; and the only point to which very special attention in staying should be given in making boilers is the staying of the flat surfaces. It is evident from the results which Mr. Boyd has obtained that we have now at last found ourselves in a position to make marine steel boilers. The only question remaining to be proved, and nothing but time can prove that, is how long they will last—whether they will be more or less liable to decay than boilers made of common iron.

Mr. MICHAEL SCOTT (member). I have had some specimens of steel sent from Sheffield by Mr. Thomas Hampton, from the Phœnix Steel Works, which may interest some members more particularly with regard to the question of welding. The process is intended for armor plates, but would be applicable for plates for other purposes. Ingots are to be cast in the ordinary way. They are then hammered into slabs and rolled into thin sheets, and in the specimen shown as many as one hundred and twelve sheets were welded at one operation. This would be done in an armor-plate furnace by passing them through the armor-plate rollers. In this specimen you can see the laminæ quite distinctly. He proposes then to hammer the surface of the armor plate which is represented by this end of the specimen. Off that end a portion has been cut, and that has been subsequently punched cold in the direction of the reed of the laminæ without the slighest appearance of injury to the metal. I understand he also proposes to employ it for boiler plates as well as armor plates; and he adds that the cost of the operation would leave the armor plate at about the same expense as the iron, the great difference being the diminished waste in the steel during the manipulation as compared with the iron.

The following communication from Mr. Hampton was handed in by Mr. Michael Scott:

HAMPTON'S PATENT TOUGHENED STEEL ARMOR PLATES.

PHŒNIX BESSEMER STEEL WORKS,
The Ickles, Sheffield, April 3, 1878.

The steel may be manufactured either by the 'Bessemer or Siemens' process (the former preferred), and the raw materials are selected from the best brands of Swedish and English hematite pig irons, together with ferro manganese or spiegeleisen.

The steel contains only a very minute proportion of carbon, and is comparatively free from phosphorus.

The ingots are cast in ordinary molds, and after being hammered, or cogged by rolling into slabs, are then rolled into thin sheets or plates for the purpose of imparting toughness and tenacity to the metal. The sheets or plates so prepared are afterwards piled in the armor-plate furnace for the purpose of heating and welding—"the latter process being effected by passing the pile through the armor-plate mill rolls. Precisely in the same manner as ordinary wrought iron armor plates are produced."

The usual difficulty in welding steel is obviated by reducing the slabs into *very thin layers* as previously described, and, with ordinary care, will stand as much fire or heating (without in the least deteriorating the quality of the steel) as wrought iron.

After the armor plate is rolled to within about 2 or 3 inches of the thickness required, it is then hammered to finished dimensions by means of a steam hammer, "the latter process being intended to give additional density to the surface of the steel, and thereby render the resistance to penetration more effective."

The cost of manufacturing steel armor plates by my method does not exceed the present cost of making wrought-iron plates of any thickness for ship's armor or coast defenses.

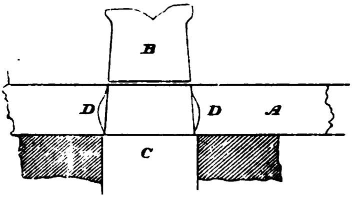
THOMAS HAMPTON.

Dr. SIEMENS (associate). I think this paper of Mr. Parker's has brought before us some very important matter, and the questions arising upon it have been so well discussed already that I feel it is almost unnecessary for me to say anything.

The CHAIRMAN. There is a question upon which you can give us important information, namely, whether the thing called steel and the thing called iron exist any more as separate existences.

Dr. **Siemens.** Chemically speaking the only difference is that what we call mild steel is pure iron—it is more essentially iron than what is commonly sold as such; but as regards the physical properties of the material, it differs in many points from iron and ought not to be confounded with it, because if we were to call it iron and confound it with iron, we should think it right to treat it in the same manner, whereas it requires in many respects different treatment. In fact, Mr. Parker has given you one or two instances. I had occasion to allude at another place to one point which was brought before my notice very forcibly with regard to the steel boilers which Mr. Boyd has constructed. After testing samples of the plates that were supplied by the manufacturers the rumor was spread that the material was wholly unreliable—that instead of breaking through the riveted seam, through a narrow strip of metal, it broke through a line of 50 per cent. greater extension rather than through the narrow place; and as iron would not have given way in that manner, and as the experience of engineers has been derived from iron, it was at first concluded that the new material was not reliable; but it was soon discovered upon further investigation that the method of riveting employed had been such as to be unsafe for this mild steel. Two large rivets had been put in a forward position and other rivets to make up the holding strength between the piece under test, and the shackles yielding were put back. In dealing with a material like this mild steel, the strain comes entirely upon the forward rivets, whereas the backward rivets being, as it were, the elongation of a much larger piece of metal, do not feel the strain at all. As it appears to me these two forward rivets were near the edge of the metal and they set up a tearing action. Now, it was remarked that although the sample was expected to elongate 25 per cent. before rupture took place, it did not elongate at all, but broke in the manner already alluded to.

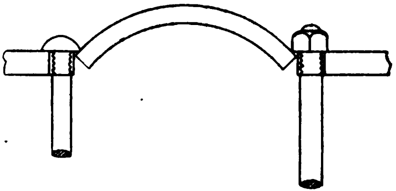
In further testing this metal it was found to be perfectly homogeneous and capable of an amount of elongation of 25 or 28 per cent. before breaking. If you take a strip of India rubber you may elongate it to several times its length, but if you make the least nick in the edge of the India rubber and try to elongate it, it will tear readily. In the same way if you give this mild steel a chance of tearing it will tear. In that case no elongation will take place, because in tearing the strain is confined to an infinitesimal amount of area, and its course in fracture will be the result not of the total amount of resisting area, but of the direction the tearing action may chance to take. With regard to riveting, Mr. Parker states, and perfectly correctly I need hardly say, that in punching a plate such as is used in the construction of large boilers, the metal in the act of punching is reduced 30 per cent. in strength. Mr. Kirk has already offered an explanation which is similar to the explanation I should give; but with your permission I will draw attention, perhaps a little more, to the precise action that I conceive takes place in punching a thick plate of this material. If you will allow me I will make a little sketch. Suppose A to be the plate to be punched, B the punch, and C the supporting die. As the punch penetrates into the metal, the latter does not immediately sever; it will first yield by compression, and it will as it were be forced laterally into the mass of metal surrounding the punch, where it will create the zone of highly compressed metal represented by the lines D D in the sketch; the metal beyond this zone of compression will be to a certain extent under tension. If the piece punched out is afterwards examined it will be found to be about 10 per cent. thinner than the original thickness of the plate, and that diminution in thickness is accounted for by these forces of compression and extension set up within the metal. If the plate is now subjected to a tearing strain, it has to resist besides this, the abnormal states of strain into which it has been thrown by the punching action, and is therefore destroyed with a force considerably less than that it can resist in its original condition as the paper shows. Now, if this plate after the punching has taken place is subjected to annealing, then this strain between metal and metal will be relieved and the plate will regain its full strength; or, if this strained metal D D be rimed away and the hole be enlarged the plate will have its full strength. But I have the results of some experiments which were made by Mr. Riley at the Landore Works, near Swansea, proving that punching does not weaken the plate, but increases the strength of it. His experiments were not confined to one or two cases, but comprised a whole series that, strange as it may appear, went to prove that the punching absolutely increased the tearing strain of the punched plates, without annealing, to something beyond the strength of the plate per square inch before it was punched. I should first say he used the precaution of making the die below the punch larger, to perhaps a greater extent than is usually done, and the result was, to my mind, that the mass of metal under compression of which I have spoken was not formed, and that the plate was thus relieved from the abnormal strains generally produced in punching. Under these circumstances I can understand that a slight increase of strength should take place for the same reason that if you subject a bar of metal of any form to strain its strength will be increased. It is a subject perhaps not sufficiently understood, but I have had an opportunity of observing very extraordinary results in that way. Take a bar of iron or steel and subject it to a strain exceeding the elastic limit, and allow this strain to be active for some time, in testing the same bar again you will find not only the elastic limit has been raised, but that the ultimate breaking strain is considerably increased. In that way whatever



strain you put upon a piece of metal you increase its strength, unless the strain is applied in such a way as to set up contending forces within the metal. Mr. Parker has alluded to two forms of construction with regard to marine boilers. The one is the admirable furnace of corrugated form which is represented by the drawings, and which I quite believe is a step in the right direction. The other is an experiment of construction which I had occasion to make, and which, by Mr. Parker's request, I have put before the meeting in drawing, Fig. 3. I was asked to design a vessel that could resist the pressure of at least 1,000 pounds per square inch, and with a capacity of not less than 100 cubic feet. The mode in which such high-pressure air vessels have been hitherto constructed has been in making them multitubular; but in resorting to that construction you get many joints and you have also a voluminous structure. It occurred to me that a better result would be gained by rolling cylindrical bands of metal from solid steel ingots—cylinders of 1 foot in depth, and, as this drawing would show, 44 inches in diameter. In rolling metal in this fashion you get the whole strength of the metal in the right form for resisting a bursting strain. The difficulty was to make joints between the cylinders, and this was accomplished by turning in the face of each a **V** groove of about ¼ inch deep; rings of copper wire of ⅜ inch thickness are thereupon placed between groove and groove, and the whole set of cylinders are screwed up and bolted together by sixteen or twenty long bolts made of steel of great elasticity containing about ½ per cent. of carbon, with a resisting power of about 50 tons to the square inch on the ends of the vessel. After screwing down these bolts hydraulic pressure was applied, and the vessel stood 1,300 pounds to the inch without a leak, but at that point a great many joints began to weep. The bolts were then drawn up one-eighth of a turn, when the vessel was found to be perfectly tight at the pressure of 1,300 pounds, but the joints began to weep again at 1,400 pounds. We might have continued the operation of drawing up and have reached a higher limit of pressure, and seen whether all the parts of the vessel were not strong enough to resist 2,000 pounds to the inch, but inasmuch as the vessel in use was only to resist a pressure of 1,000 to the square inch, it was thought sufficient to tighten these bolts to the limit of 1,400 pounds, because we felt that if ever the pressure should reach that point the bolts would yield, owing to their elastic property, and allow the fluid within to escape, thus avoiding the risk of an explosion taking place. In that way vessels I believe can be constructed which are perfectly safe and capable of resisting a very high pressure. I may mention that in this form steel will bear a pressure of 45 tons to the inch with perfect ease and safety. It occurred to me therefore that marine boilers might be constructed on the same principle, by rolling a number of rings in this fashion and placing them one behind the other, with these copper rings inserted and bolting the whole down. In that way we should have at any rate a boiler shell capable of resisting 200 or 300 pounds pressure without the least risk of an explosion, whereas in riveting very heavy boiler plates of an inch in thickness there is always some difficulty in treating the material.

The **Chairman.** Gentlemen, I am sure you will all agree with me that we have had a most interesting, and allow me to add, most profitable discussion. I am sure we are all very grateful to the author of this paper, and he will allow us to thank him for having given us so valuable a contribution, and so good an example for the future. I am sure that we are equally indebted to those gentlemen who have been kind enough to take part in this discussion. I hope you will not think that I have devoted too much of your time to the subject, because I think that we have got some very valuable information. I feel also that a very important remark was made by Dr. Siemens on this matter of punching steel plates, and as in that he expressed only a part of what is my belief in regard to it, I would call the attention of the author of the paper to a question as to the mode of punching steel plates which I should not have troubled you with unless Dr. Siemens had hit on exactly the same idea. I will take this plate of Dr. Siemens, as he has drawn it, and I will suppose that you wish to punch that plate without injury to the steel. Allow me to tell you that I made all these experiments many years ago with iron for the purpose of using the iron without any injury, but I found the iron was injured by injudicious punching. I then tried to find out how I could punch the iron without injury. In order to do that I began to punch very thick plates with very small holes. Now I will tell you the result. The result of punching very little holes through a plate with sufficient power was that the plate always parted in a cone, spontaneously and freely. I therefore said to myself, surely if this mode of treating the plate in a cone is the mode which is most agreeable to the iron, and which leaves it in the best state, it would be wise to use that as the form of our punching holes, and it would be wise next to rivet our rivets into a hole of that shape. I now therefore venture to suggest to you to do the same thing in steel which I found of the greatest value in iron—namely, I ask you to make your punch of a given size and of a certain shape, to make your opening on the other side of a larger size—different degrees for the different kinds of metal—and my opinion is that you will arrive at some very beautiful cone which will be the best for each kind of steel you use; and my opinion is also that if you will take this opening and put a good rivet head there, and then simply well build up this hole with the point of the rivet, in the course of the riveting of the red hot rivet you will get everything in the most beautifully annealed condition immediately after, and you will have no more trouble on the point. I do not say you will find out this, for I am not a prophet, but I say that I found it so with iron, and I would seriously recommend it to your consideration in your experiments. I can also give you one more little piece of information, in reply to a question that was asked; we have had very important experiments given to us on the construction of boilers made of flat plates united by square stays—stays square to their direction. Now allow me simply to say to you that I have tried boilers of the one-sixteenth part of an inch thick, stayed at 3 inches apart with stays, I think, ⅜ inch minimum diameter. I have put upon these nuts, inside and out, with washers, and I have proved these thin boilers to 240 pounds pressure, and found them perfectly safe. Owing to the thinness of the metal steam could be raised extremely rapidly, from the little impediment offered by the thin metal; and owing to the coolness of the metal produced by the vicinity of the cool water to the hot surface of the metal I have found them last for years without the slightest corrosion. Those are the only remarks that occur to me to add to what I consider a most valuable discussion, for which I feel sure we are all extremely obliged. If there are any points upon which Mr. Parker could give us a conclusive answer we should be still more indebted to him.

Mr. **William Parker** (member.) The chairman and Dr. Siemens have expressed themselves strongly in favor of employing a larger bolster in punching steel plates than is usually adopted, and while I concur generally in the statement that slightly conical punching produces less injury on the material than directly parallel holes, my experience does not quite bear out the supposition that the loss from punching diminishes exactly in proportion to the acuteness of the cone. In the series of comparative experiments to which allusion was made in my paper we employed just an ordinary punch, but used bolsters of various proportions—in fact we went in some plates so far from the parallel as we could without the hole becoming jagged, and the result was that the loss to the material by taper punching was about 26 per cent. as compared with 33 per cent. That no doubt is a considerable difference, and gives some ground for the belief that by the introduction of special patent punches we may ultimately be enabled to reduce this loss to practically nil; but still I think it would be misleading to assert without qualification that conical punching is entirely free from the injurious effects produced by ordinary punching. No doubt the circular shell plates of marine boilers will come to our hands rolled in one piece, but I do not think the arrangement of Dr. Siemens will be eventually carried out in its entirety. These long stays are no doubt admirably suited for a vessel to withstand water or air pressure, but when variations of temperature have to be taken into consideration, the joints would probably be found to leak in consequence of the unequal expansion and contraction. With regard to the question of stayed surfaces which has been raised by Mr. Wright and Mr. Kirk, and so fully gone into by Mr. Boyd, I may just give one or two particulars of the experiments we made. They were conducted in the first instance to ascertain the resistance of steel to buckle as compared with iron. We made the stays with riveted heads, the pressure was applied until the plate took something like the shape shown in the accompanying sketch, so that



there was nothing left holding it except the small piece of head. If, instead of a riveted head, a washer and a nut were used, that would practically be much stronger; but every one who has had experience at sea with nuts in furnaces and combustion chambers knows that they give trouble, and last only a short time. The important point, however, is what is the most desirable distance to pitch these stays apart so that the plate may resist buckling, and retain the same factor of safety as the other parts of the boiler. As temperature must be taken into consideration in arriving at this conclusion, I have not before referred to these experiments, because, like Mr. Boyd's, they were not conducted under the influence of temperature, but when they have been so conducted as to give a really practical result I shall not fail to make them known. I was present at the experiment made with plain and corrugated furnaces to which Mr. Wright referred, and the corrugated flue expanded something like one-fourth of an inch before it collapsed. Had its ends been attached as they would be in a boiler, I am inclined to think it would have resisted a greater pressure than it did on that occasion—the plain furnace did not elongate at all; if it had been secured at the ends it might also have stood a little more pressure. I am sure that these experiments prove that we have not had the factor of safety in plain flues that we supposed we had. As to the corrugations forming a receptacle for deposit, perhaps the best answer I can give to that is, that gentlemen who have had for ten or twelve years forty or fifty steamers to look after, and whose reputation depends on keeping them in good order with little expense, are confident that no trouble will be experienced from deposit, and are fitting several of their steamers with these furnaces; however, I shall from time to time have the opportunity of examining these furnaces, and shall not fail to make known the actual results, experience alone being, in my opinion, the only means of solving this question.

Mr. Kirk. Will you allow me to say one word? Mr. Parker spoke of the thin plates being less affected by punching than the thick ones. I presume the thin plates and the thick plates were all punched by the same diameter of punch?

Mr. W. Parker. No; different diameters.

Mr. Kirk. Was the diameter of the punch in proportion to the thickness of the plates?

Mr. W. Parker. Yes, but not in exact proportion.

A FEW YEARS' EXPERIENCE OF THE SCREW PROPELLER: ITS LESSONS AND RESULTS.

BY ARTHUR J. MAGINNIS.

[Read at the twentieth session of the Institution of Naval Architects, April 3, 1879; N. BARNABY, Esq., C. B., vice-president, in the chair.

Of all the numerous matters connected with steamships which have engaged the attention of scientific men, that of the screw propeller seems to be the most intricate and incomprehensible.

A study of the numerous papers and articles which have been written, reveals how widely opinions differ on its many subjects, and also points out the strange fact that experience (the most perfect instructor in other steamship matters) has, up to recently, taught but little of the proper proportions of pitch, surface, diameter, or shape, although it has given severe and costly lessons of the great strength required both in the various sections of the boss and blades, and in the mode of fastening it on the propeller shaft.

A brief glance over a few years' experience and study of the various propellers, and their effect on the steering of large ocean steamers, may perhaps throw some light on this great engineering enigma, and afford some information of practical use.

The construction of the propeller has of late undergone some change from the old style of a solid casting, owing to the great increase in the size, which entails considerable difficulty in obtaining cast-iron ones of correct pitch and sound metal, a large cast-iron propeller of suitable strength weighing from 21 to 25 tons.

The mode of construction now most in favor is that of having a boss with the blades bolted to it by flanges, as shown on Fig. 13, plate V. This mode for propellers of 16 feet diameter and upwards is superior to the solid cast-iron ones, although there is the great objection to it that the first cost is from two to four times greater; but notwithstanding this drawback it is gradually superseding the old style.

When this method first came into use objections were raised to it, owing to some of the bosses having split or broken, as shown on Fig. 13, plate V, and also on account of the bolts breaking, which caused the blades to be lost.

Owing to the failures of the earlier cast-iron bosses, other metals have been tried, such as gun-metal, brass, steel, and in one instance wrought iron, but with the exception of the latter the great expense will always prevent their general use, as a moderate-sized boss of those materials would cost from £90 to £140 per ton, or say from £600 to £1,000 for many sizes at present in use of cast iron.

The difficulty of obtaining a sound forging of 4 or 5 feet cube has, up to quite recently, prevented wrought iron being used, but the one in use at present, which is of the largest size, has given excellent results, and the price is not so high as to be a serious objection, being about 25 or 30 per cent. more; however, cast-iron bosses have proved to be most satisfactory, and when designed with a due regard to the sectional strength will be found most suitable and reliable.

The ordinary square-edged flange of the blade shown on Fig. 13, Plate V, being considered most objectionable on account of its increasing the resistance in revolving through the water, Messrs. Vickers, of Sheffield, have introduced a very neat and suitable style of boss; this arrangement, as shown on Fig. 12, Plate V, consists in having an angular recess in the boss into which the flanges of the blades are neatly fitted, so that the fillets of the four blades, together with a small portion of the boss, form a complete circle.

The bolts attaching the blades to the boss have been the cause of very great expense and trouble, but fortunately the difficulty has been got over by the use of larger sections and best quality of steel; the nuts are now made of either steel solid-ended, as shown on Fig. 12, Plate V, or of the best Lowmoor iron, open-ended and covered with Portland cement to preserve them from the action of the water.

In coming to the subject of the blade of the propeller, the first thing to strike one is the fact that notwithstanding the numerous patented forms which have been brought out, the plain flat or oval-faced (on horizontal section) blade introduced by Mr. Griffiths some years ago, is still the most in favor. This form is almost the only one in use on the large steamers sailing from Liverpool, after many trials of patented forms, a fact which would lead one to believe that, taken altogether, it possesses most of the various qualities requisite for a serviceable propeller.

The blades of the propeller have been made of almost every possible shape, form, material, number, and pitch; one proposing that they should be bent forward; another, that they should be bent backward; another, that the point of the blade should be in advance of the root; another, that the root should be in advance; another, that the blade should be straight; another, that the horizontal sections of the blade should be concave; another, that they should be convex; another, that they should be the shape formed by an actual screw-thread; another, that they should have a large diameter; another, that they should have a small diameter, and so on, until the owner or engineer of a steamship knows not which to act upon or what to do; and the strangest thing of all is, that they are supported by tabulated statements and records showing the superior advantages of the so-called improvements.

Fig. 10, Plate V, shows various shapes which were successively tried and found to be unsuitable, as their forms necessitated a thick medial section to prevent them breaking at or about the parts marked, as was the case with many. Another serious defect was the great vibration caused by them. Several of the principal styles of patented propellers have also been tried, but when the propeller required renewal they were given up on account of not giving such superior results as would compensate for their extra expense and difficulty in making.

Of all the numerous shapes of blade tried up to the present time, that shown on Fig. 9, or slight modifications of it, has proved the most satisfactory, and is now almost the only shape in use on the transatlantic steamers. The advantages of this form are, that it has proved to possess the required strength with a moderate sectional thickness, it is thoroughly reliable in all weathers, and has lessened the vibration considerably.

Concerning the question of the number of blades a propeller should have, numerous trials have been made. The results given by the two-bladed propeller are that they drive the vessel well, but with almost every form of blade they cause vibration, and there is the decided objection to them of being easily disabled. Three-bladed propellers have also been tried, and have been found to be altogether unsuitable for large, fine-lined steamships, as they cause excessive vibration. Six blades have also been tested, but gave no improvement, so that under all conditions the four-bladed propeller has been found to answer the purpose best, and is consequently the most in favor.

We now come to the much-vexed questions of pitch and diameter; the relative merits of pitches coarse or fine, uniform or varying, or of diameters large or small, have long been subjects of serious consideration to marine engineers. The writer has, during the past few years, endeavored to obtain some reliable statistics bearing on them, as propellers of both different pitches and diameters have been used on the same steamers directly under his notice. The following tables, which have been compiled from the logs of three of the largest transatlantic steamers, will show in a brief form some interesting information; the voyages made with the different propellers being placed together when they have been made under similar circumstances, that is, the same length of time out of dry dock, the same state of weather, displacement, &c., as near as possible, so as to show the fairest comparisons under the many circumstances which materially affect the screw propeller. In addition to these, the results obtained from H. M. S. *Iris*, which are the most remarkable experiments yet made, are also given, similarly arranged.

STEAMSHIP ALPHA.

Large Propeller: Diameter, 23 feet 6 inches; pitch at boss, 26 feet; at periph., 33 feet; mean, 30 feet; surface, 124 square feet.
Small Propeller: Diameter, 21 feet; pitch at boss, 29 feet; at periph., 30 feet; mean, 29 feet 6 inches; surface, 153 square feet.

Voyage.	OUTWARD VOYAGE.								HOMEWARD VOYAGE.							
	Average revolutions per minute.		Average slip, per cent.		Average speed, knots per hour.		Total consumption, tons.		Average revolutions per minute.		Average slip, per cent.		Average speed, knots per hour.		Total consumption, tons.	
	Large.	Small.	Large.	Small.	Large.	Small.	Large.	Small.	Large.	Small.	Large.	Small.	Large.	Small.	Large.	Small.
A	53.0	54.8	11.9	6.1	14.7	15.0	873	905	52.7	54.8	11.7	6.1	14.7	15.0	925	825
B	53.5	56.1	10.5	9.8	14.1	14.7	1,016	915	53.4	54.8	6.6	6.7	15.0	15.0	940	885
Mean	53.25	55.4	11.2	7.9	14.4	14.8	944	910	52.5	54.8	9.1	6.4	14.8	15.0	932	855
Home	52.5	....	9.1	....	14.8	....	932	....	Out.	55.4	....	7.9	....	14.8	....	910
Round voyage	52.8	....	10.1	....	14.6	....	1,876	....	....	55.1	....	7.1	....	14.9	....	1,765
RESULTS.																
Large propeller									52.8		10.1		14.6		1,876	
Small propeller									55.1		7.1		14.9		1,765	
In favor of small									....		3.0		0.3		111	

STEAMSHIP BETA.

[Propellers as above.]

A	52.4	56.1	9.1	8.8	14.8	15.6	874	810	52.3	55.4	8.9	6.3	15.4	15.4	928	845
B	51.8	54.6	14.5	12.3	13.8	14.7	955	833	51.0	54.9	12.0	11.8	14.4	14.6	759	902
Mean	52.1	55.4	11.8	10.5	14.3	15.1	915	822	51.6	54.6	10.4	9.0	14.9	15.0	844	874
Home	51.6	....	10.4	....	14.9	....	844	....	Out.	55.4	....	10.5	....	15.0	....	822
Round voyage	51.8	....	11.1	....	14.6	....	1,759	....	....	55.0	....	9.7	....	15.0	....	1,696

RESULTS.

Large propeller	51.8	11.1	14.6	1,759
Small propeller	55.0	9.7	15.1	1,696
In favor of small	....	2.4	0.5	63

STEAMSHIP GAMMA.

LARGE PROPELLER: Diameter, 21 feet 6 inches; pitch at boss, 29 feet 6 inches; at periph., 32 feet 6 inches; mean, 31 feet; surface, 128 square feet.
SMALL PROPELLER: Diameter, 20 feet; pitch at boss, 27 feet 3 inches; at periph., 28 feet 7 inches; mean, 28 feet; surface 124 square feet.

A	45.9	52.6	12.0	9.6	12.3	13.1	777	627	46.8	51.6	3.5	2.8	13.8	13.8	672	593
B	45.4	52.9	17.4	13.0	11.4	12.1	872	755	46.2	53.4	3.6	2.7	13.6	14.3	672	728
Mean	45.6	52.7	14.7	11.3	11.8	12.5	824	691	46.5	52.5	3.5	2.7	13.7	14.0	672	653
Home	46.5	....	3.5	....	13.7	....	672	....	Out.	52.7	....	13.3	....	12.5	....	691
Round voyage	46.0	....	9.1	....	12.7	....	1,496	....	....	52.6	....	7.0	....	13.2	....	1,344

RESULTS.

Large propeller	46.0	9.1	12.7	1,496
Small propeller	52.6	7.0	13.2	1,344
In favor of small	....	2.1	0.5	152

H. M. S. IRIS.

Large Propeller: Diameter, 18 feet 6 inches; mean pitch, 18 feet 6 inches.
Small Propeller: Diameter, 16 feet 3 inches; mean pitch, 20 feet 6 inches.

Speed per hour.	Average revolutions.		Indicated horse-power.		Consumption at 2 pounds per I. H. P.	
	Large.	Small.	Large.	Small.	Large.	Small.
Knots.					*Cwt.*	*Cwt.*
12	65.0	61	2,561	1,837	45.7	32.8
16½	87.9	86	7,478	5,132	133.5	91.6
18½	....	97	....	7,734	....	138.0

IN FAVOR OF SMALL PROPELLERS.

Speed.	I. H. P.	Consumption.
Knots.		
12	724 less.	12.9 cwt. per hour = 16 tons per day.
16½	2,346 less.	42.0 cwt. per hour = 50 tons per day.
18½	....	

By these results it will be seen that propellers of small diameter have in each case proved the most economical and effective, that on the steamship *Alpha* having given a reduction of 3 per cent. slip, and 111 tons of coal on round voyage, and an increase of .3 knot per hour in speed; that on the steamship *Beta* gave a reduction of 2.4 per cent. slip and 63 tons of coal on round voyage, and an increase of half a knot per hour in speed; that on the steamship *Gamma* gave a reduction of 2 per cent. slip and 152 tons of coal on round voyage, and an increase of half a knot per hour in speed.

In the case of H. M. S. *Iris* the particulars are taken from the reports published in the Times; by these it will be seen that the smaller propellers proved much superior, having driven the vessel at a speed of 18½ knots per hour, which the larger propellers were totally unable to do; the smaller propellers also gave a speed of 16½ knots (the maximum of the larger) with about two-thirds of the indicated horse-power. The consumption on the trials has unfortunately not been published, but if we assume it to be 2 pounds per indicated horse-power, the saving of fuel effected at this speed would be about 42 cwt. per hour, or 50 tons per day, and at a speed of 12 knots about 13 cwt. per hour, or 16 tons per day.

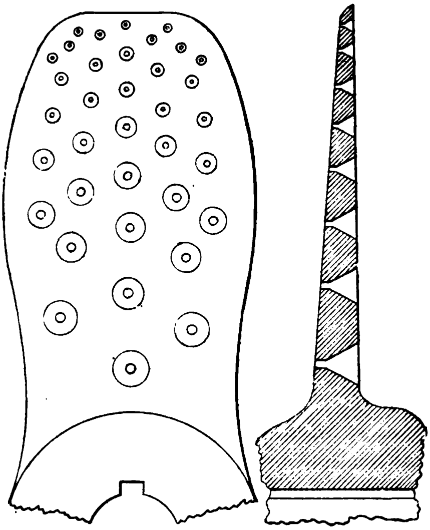
From the foregoing it is evident that the small diameters give the best results, and also (excepting in the case of the *Iris*) that the existing pitches are a little too coarse, for if the speed of piston will allow it, a slightly reduced and more uniform pitch will be found advantageous in many ways, as was found to be the case with the *Gamma*, the piston speed having been raised from 460 to 520 feet per minute by the smaller propeller, and since the increase the engineers have reported the general working of the machinery to be greatly improved.

The superior qualities of the present favorite shape of blade has also served to bring about some of the foregoing improvements, especially to reduce the vibration; it will also be noticed that the form of blades which gave such good results on the *Iris* are almost similar to those now in general use on the transatlantic steamers. The different propellers used on these occasions are shown on Fig. 9, Plate V.

The next subject to be noticed concerning the screw propeller, owing to the great expense which it causes, is the extensive corrosion which takes place on the back of the blades; and to so serious an extent does this corrosion take place that it renders the life of a large cast-iron propeller extremely short, being only about two years and a half when in constant service. Steel blades are also affected by it, but improved steel has lately been introduced, which has been found to last from three to four years.

The cause of this corrosion has, like every other matter connected with the propeller, given rise to much difference of opinion, the most general being attributed to the vacuum at the back of the blade; but as corrosion also takes place upon the driving face of the blade, along by the leading edge, some other means must be at work to have such an effect. Having carefully examined many propellers which had been condemned owing to their corroded condition, it is the writer's opinion that the great cause of it is the formation of some acid by the mixing of the atmosphere and salt water, which attacks the iron; this mixing is caused by the descending blades of the propeller when crossing the top center, having displaced the water, and also formed a partial vacuum at the back of the blades, which then draws in water highly charged with air, otherwise foam, and if the vessel be light, pure air, which will be carried down into the water by the blades.

Many arrangements have been designed to prevent this corrosion, such as fastening zinc plates to the backs of the blades, and also iron plates. Covering the blades with a coating of vulcanite has been tried, but it would not remain on, owing to the friction. A Portland cement coating has also been used, but with no better success. Another design, which has recently been brought out in New York to prevent the corrosion, is to perforate the blades of the propeller with holes (as shown on sketch), the diameters of which decrease as they approach the circumference; the holes are also countersunk on the driving face of the blade. Numerous other advantages are claimed for this improvement, which, if on trial prove effectual, will more or less revolutionize screw propellers. The usual course of the corrosion is shown on Fig. 10, Plate V, the blade A showing the driving side and B the back side, the darker parts being those where the corrosion is quickest.



The mode of attaching the propeller to the shaft is a point of which it cannot be said that experience has taught nothing, as almost daily accidents occur which show the necessity of great care being used when fitting on a propeller. If the boss be not a perfect fit on the shaft, it is not to be expected that the keys will be able for their work, nor is the propeller nut to be depended upon to keep tight.

Several cases have come under the writer's notice during the last few years, of disablement through apparent defect in the keys, whereas if the boss had been carefully fitted the keys would have been found amply strong. One of these cases was that of a new cast-iron propeller, which had been put on with the old keys. On the return of the vessel the propeller was reported loose on the shaft, and on examination it was found that one key was sheared across, and that the boss had been working to a serious extent; after this a careful fit was made and keys of the same dimensions fitted, with a result which proved they were of ample section. The method often adopted of putting the keys at right angles is open to serious objections, owing to the great difficulty they present of making the boss a proper fit, and should never be adopted with large propellers, but where there are two keys, which is always desirable, they should be placed opposite to one another. Another danger in the fastening of propellers is that of allowing too short a length of key, which renders it liable to be sheared across or to fracture the boss; the latter defect is often found to take place when the propeller boss is cored out in the inside, and the key only fitted in the forward end, as is generally the case.

The nut securing the boss on the shaft has in a few instances given trouble by slacking back and injuring the rudder-post. To avoid this the nuts should in all cases be fitted on a screw of an opposite hand to the propeller; that is, if the propeller be right hand, the nut should be fitted left hand, and *vice versa.*

At the present time much difference of opinion prevails as to the question of solid cast-iron or movable-bladed propellers; on this question the following incidents which have occurred will perhaps be of some service. Some time ago, a large Atlantic steamer, when shifting docks a few days before the date of sailing, fouled the dock wall with the propeller, and broke off one of the blades. This accident, though slight in itself, was enough to prevent her sailing, as to have a new propeller she would require to go into dry dock, whereas had it been a movable blade it could have been replaced when the vessel was afloat, and thus have allowed her to sail at the appointed time. Another case was that of a large steamer on the South Pacific coast, which broke her propeller, and was detained some weeks until a caisson to fit the stern was built so as to get at the propeller, as there was no dry-dock large enough to admit her. On the other hand, another steamer, with a movable-bladed propeller, broke all the blades when within a few days' sail of a Brazilian port, into which she sailed and replaced the blades by lightening the ship aft, the delay being one week, of which only two days were occupied in taking off the old stumps and putting on the spare blades which she had on board, the other days being occupied in discharging and replacing cargo. Other objections to solid propellers are their great size, which occupies so much cargo space when carried as spare, and in case of breaking off one blade the entire propeller has to be condemned; and again, an entire propeller has always to be kept in reserve, as it occupies so long a time making, say from eight to nine weeks; so that although the first cost of the movable style is from two to three times as expensive as the solid, the great advantages they possess of quickly and easily effected repairs (which prevent serious delays) have proved them to be the most useful. The table showing the particulars of the propellers of some of the largest steamers sailing from Liverpool will show the general practice and dimensions now used.

Particulars of propellers of Liverpool steamers.

Name of steamer.	Diameter.		Pitch.						Surface.	Number of blades.	Material.		Weight.		Right or left hand.	Diam. of cylinders.				Stroke.		Revols. per min.	Speed per hour, knots.	Shape.
			At boss.		At perip.		Mean.				Boss.	Blades.				H. P.		L. P.						
													Tns.	Cwt.		No.	Dia.	No.	Dia.					
	Ft.	In.	Ft.	In.	Ft.	In.	Ft.	In.	Sq. ft.											Ft.	In.			
Ibera	21	0	25	0	29	0	26	4	111	4	Steel	Steel	18	1	Right	1	56	2	78	5	0	52	14	Ordinary Griffiths
City of Berlin	21	0	31	0	31	6	31	3	130	4	Cast iron	Cast iron	20	...	do	1	72	1	120	5	6	55	15½	Do.
Britannic	23	6	26	0	33	0	30	0	124	4	Wrought iron	Steel	21	13	do	2	48	2	83	5	0	52	15½	Do
Bothnia	20	10	26	6	29	7	28	1	113	4	Cast iron	do	15	10	do	1	60	1	104	4	6	52	13½	Do.
Ohio	17	0	...		...		24	0	...	4	...	...	...	...	do	1	57	1	90	4	0	60	13	Do.
Sardinian	20	0	24	0	26	6	25	6	107	4	Cast iron	Steel	16	...	Left	1	60	1	104	4	0	54	13½	Do.
Gallia	21	0	26	0	33	0	30	0	116	4	do	do	21	7	Right	1	61	2	84	5	0	56	14½	Do.
Spain	20	6	Uniform.				26	0	120	4	do	Cast iron	21	4	Left	1	60	1	106	4	6	51	12½	Do.

Of the many and varied ways in which a propeller may become disabled, it may be of interest to mention a few of the most remarkable. In 1873 the *Celtic* fouled some floating wreckage off the south coast of Ireland and lost the entire propeller. In 1874 the *Scythia* struck a large whale and broke off three blades. In 1877 the *City of Berlin* became disabled by the keys in the propeller boss having fractured the boss to their own depth, or in other words, turned out the metal as shown on Fig. 11, Plate V. Another peculiar disablement of a propeller is shown on the same drawing. This occurred to the *City of Chester* in 1878; as may be seen, the propeller broke through the boss, losing two of the blades, but the fracture not being large enough at the after end to clear the shaft's diameter, it hung in its place until the vessel arrived in port.

Throughout the year 1878 there were upwards of thirty-three cases of disabled propellers reported in one of the Liverpool daily papers (not a shipping), and no doubt this would be but a small fraction of the large number which had actually taken place.

Attention has been called in another portion of this paper to the square-edged flanges of the blades, owing to the great power absorbed in driving them through the water; another serious addition to them is the thick medial sections of the blades, which have of late years been made extra thick owing to the many breakages. It is to these points attention must be turned in the future, in order to increase the efficiency of the screw propeller by an improved mode of attaching the blades, which will allow of reduced size and different shape of boss, and the adoption of a reasonably-priced metal of great strength combined with lightness, which will allow of a great reduction in the sectional thickness. A step in the right direction has been taken by Mr. Howden, of Glasgow, who some time ago, in a paper read before the Institution of Naval Architects, brought forward in a most lucid manner the great loss occasioned by the present form of propellers owing to the foregoing defects, and has designed a new principle of construction which bears about it a look of being up to its work, although men long experienced in the subject have stated that it looks weak about the root of the blade; still, when the great reduction in the useless area and the thin sections of the blade be considered, it may prove amply strong, a fact which a short trial on a large steamer would soon show.

As almost everything in the matter of speed, safety, and prestige which large steamship companies and owners value so highly rest upon the propeller being thoroughly reliable and efficient, it may be of interest to note what experience has taught as to the design of large propellers with the metals and forms in use up to the present time, rejecting the various ones proposed; for so much depends upon it, owners are exceedingly averse to trying experiments; as one of them remarks, "They prefer the safe to the scientific."

For propellers of 16 feet diameter and upwards, the system of having the boss and four blades separate is the best, the former being made of superior cast iron annealed, care being taken not to have it cored out too much, as this has caused the failure of many. The blades should be of the improved steel recently introduced, of the shape shown on Fig. 9, with a good substantial section, the thickness at the root being $\frac{1}{2}$ inch to $\frac{9}{16}$ inch for each foot of diameter; about 2 inches in from the tip the thickness should be about 1 inch or $1\frac{1}{4}$ inches. The points of the blades should be bent from 2 to 3 inches aft of the straight or perpendicular line of medial section, commencing about 2 feet 6 inches from the tip; the horizontal sections on the back of blade being convex, of increasing radii as they approach the tip, and on the driving or after face slightly convex at the root, gradually decreasing till they become straight. The pitch should not vary more than 2 feet at the most, and should be fine enough to allow the engines to work at a piston speed of from 500 to 560 feet per minute. However, these statements may be subject to many conditions, and from the strange results which have of late been obtained from propellers, and which have overthrown the long-recognized rules and theories, it seems to the writer that on this subject no stated rules can apply, as almost every steamer requires a certain propeller, which unfortunately only well-watched trials will show or reveal, but it may be safely said that the engines as a rule are at present overweighted or overscrewed, the pitches being slightly on the coarse side and the diameters unnecessarily large.

We now come to the question relating to the propeller which has of late been most before the public, that is, the effect on the steering of the steamer. From a short experience and study of this important subject, the writer is of opinion that the effect is contrary when the steamer is starting or moving slowly ahead to that which has been generally stated and accepted. It has often been stated that steamers with right-handed propellers would (if the helm were kept amidships and a flat calm prevailed) go round in a circle to the right hand (or starboard), and left-handed propellers in a circle to the left hand (or port). The diagrams with the following explanations, will serve to render more clearly the opinions for the contrary being the case, when the steamer is just starting or moving slowly ahead, *i. e.*, that a right-handed propeller will make the ship describe the circle to the left hand and *vice versa.* Plans of vessels with right and left handed propellers are shown with arrows pointing to the courses which each would take, and under two of these plans are vertical elevations of the propeller looking forward, the water above the center line of shaft on each diagram being shaded light and that below dark for the sake of distinction; Figs. 1 and 2, Plate V, being the first case, when the steamer is just starting, and Figs. 3 and 4, Plate V, the second case, when the steamer is going ahead at a moderate or full speed. In the first case it may be seen that the water acted on by the top blade A has a most roundabout way of getting to the screw port, and is consequently much disturbed; so that the resistance to the blade is greatly decreased, as is also that to the ascending blade D, which owing to the decreasing density, is gradually inclining to lift or push the water before it. Then, turning to the other blades, it may be seen that the lower blade C is thrusting against the most dense and least disturbed water, and the descending blade B is gradually meeting with increasing resistance; the consequence being that as the resistance to the lower blades is much greater than that to the upper, the center of shaft, being the center of effort, will be inclined to move in the same direction as the upper blades, which have the lesser resistance (this direction is shown by the arrows); and as the stern of the ship contains or holds this center of effort, it must of necessity tend to go in the same direction, which, in the case of the right-handed propeller, will be to the right hand, so that the vessel's head will go off to the left hand (or port). In the case of the left-hand propeller, for the same reasons, the stern of the vessel must of necessity tend to go to the left hand, and thus make the vessel's head go off to the right hand (or starboard).

In brief nautical language the result is, when steamers are just starting, those with right-handed propellers will answer their starboard helm quickest, and those with left-handed propellers their port helm. Then, taking the second case, when the steamer is going at a moderate or full speed ahead, it will be seen that the water above the center line of shaft has, as Professor Reynolds expresses it, obtained an onward motion in the same direction as the vessel herself, and consequently, owing to this, the water presents to the upper blades of the propeller a much greater resistance than to the lower, so that in this case the directions of the arrows on the vertical elevations will be exactly reversed, and therefore the vessel's head will, if a right-handed propeller, go off to the right hand (or starboard), and if left-handed, to the left hand (or port). In nautical language, steamers going at moderate or full speed ahead with right-handed propellers will answer their port helm best, and those with left-handed propellers their starboard helm best. On the other hand, or third case, when the engines and vessel are both going astern, either slowly or at full speed, the effect of a right-handed propeller is to draw the steamer's head to the right (or starboard), and of a left-handed one to draw the steamer's head to the left (or port), that is, provided there is no wind blowing, and that the rudder is kept amidships, for if it be moved it will produce strange results, which I hope to point out later on.

The most important matter, however, concerning the action of the propeller on the steering is the effect brought about by the sudden reversing of the engines from full speed ahead to full speed astern, the vessel still having considerable headway on, as is the case when coming into collision, and it is under these circumstances that the peculiar effects have mostly taken attention; these may be to some extent accounted for by the fact that the water or current which acts upon the rudder is drawn by the reversing propeller in a direction directly opposite to that in which it would act upon it if the propeller were at rest, as is shown on the diagram, Fig. 7, Plate V, which is a plan of a vessel supposed to have great headway on with her helm hard a-port, and the propeller (right-handed) working full speed astern. According to this it may be seen that the real action of the rudder under these circumstances is to turn the vessel's head to the left (or port), that is, in a direction directly opposite to that which had been intended; the plan alongside of it, Fig. 8, Plate V, showing the course or direction intended, and which would have been taken had the propeller been stopped and not reversed.

This peculiar action is also assisted by the different effects of the top and bottom blades referred to before, as the vessel's head will cant most and readiest when the rudder and propeller are acting in conjunction, or, to speak more plainly, when the rudder is placed to cant the vessel's head in the same direction as the propeller would turn it.

These statements are to some extent borne out, if the reports of actual trials made by Prof. Osborne Reynolds on the steamer *Melrose*, in 1877, published in the *Engineering* papers, be correct. Figs. 5 and 6, Plate V, are plans showing these results. It may be seen that on the first experiment the steamer's head had turned twenty-eight degrees to the left (or port), the helm being hard a-port, and on the second experiment the steamer's head had turned forty degrees to the right (or starboard), the helm being hard a-starboard; the courses actually taken being in both cases directly opposite to that which the rudder under ordinary circumstances would have steered the vessel.

The additional effect to the steering given by the propeller's own action is shown very clearly in the second experiment by the greater angle (40°) canted to, than in the first, for in this case the rudder and propeller were acting in conjunction, that is, both tending to make the vessel's head cant to the right (or starboard). If the foregoing statements as to the effects of screw propellers on the steering of steamers be accepted, the following will be the various effects under all circumstances for steamers with right or left handed propellers, so arranged as to be suitable to place in the wheel-house, for the guidance of the commanders and crew.

EFFECT OF SCREW PROPELLERS ON THE STEERING OF STEAMERS.

RIGHT-HANDED.	LEFT-HANDED.
FIRST CASE.—*Steamer and propeller both starting ahead.*	
Propeller will drive steamer's head to port. Steamer will answer starboard helm quickest.	*Propeller will drive steamer's head to starboard. Steamer will answer port helm quickest.*
SECOND CASE.—*Steamer and propeller both going ahead moderate or full speed.*	
Propeller will drive steamer's head to starboard. Steamer will answer port helm quickest. Steamer will require a little starboard helm to keep straight course.	*Propeller will drive steamer's head to port. Steamer will answer starboard helm quickest. Steamer will require a little port helm to keep straight course.*

THIRD CASE.—*Steamer and propeller both going astern.*

Propeller will draw steamer's head to starboard. Steamer will answer starboard helm quickest.	*Propeller will draw steamer's head to port. Steamer will answer port helm quickest.*

That is, if helm be kept amidships and no wind prevails, as in a breeze the stern of a steamer will always seek the wind, no matter what may be the action of the rudder or propeller.

FOURTH CASE.—*Steamer going ahead, propeller working astern.*

If helm be kept amidships, propeller will turn steamer's head to starboard. If helm be put hard a-port, propeller will turn steamer's head to port. If helm be put hard a-starboard, propeller will turn steamer's head to starboard quickly and to a great angle.	*If helm be kept amidships propeller will turn steamer's head to port. If helm be put hard a-port propeller will turn steamer's head to port quickly and to a great angle. If helm be put hard a-starboard propeller will turn steamer's head to starboard.*

In this case the wind will not have much effect until the steamer has lost headway, and it must be noted that the position of the rudder is of paramount importance.

However, the first and fourth of these cases are, in ordinary service, the only ones which call for particular attention, more especially the fourth case, as the strange effects produced under these circumstances are almost always experienced when steamers are in danger of coming into collision.

It may not be out of place here to call attention to a fact which every steamship company and owner should take particular notice of, that is, the serious loss occasioned both to them and the engineering profession by permitting the available experience gained year after year on large merchant steamers to pass almost unnoticed and unrecorded.

In concluding this review of "A few years' experience of the screw propeller," the writer begs to return thanks for the permission granted him to bring forward information which he hopes will assist in showing more clearly what has been "its lessons and results."

DISCUSSION.

Mr. WILLIAM DENNY. Mr. Chairman, it seems to me that the paper we have just heard is rather taken up with the question of the construction of the propeller than with the question of its efficiency, and that the competition for which this paper entered was essentially a competition on the efficiency of the propeller. In the paper itself I can find no reference to the efficiency of the propeller, excepting that shown in Figs. 9, Plate V, in which the general doctrine held is, that by decreasing the diameter, and following Mr. Froude's discovery—of decreasing the peripheral friction, you will get more efficiency. But Mr. Maginnis in treating this, although he has followed the great discovery that we now all know so well, has not taken care to qualify his statements upon this point, because it must be quite evident to you that if this decreasing process were to be carried on far enough, you would come to the boss and then you would come to a very negative result indeed. Therefore it is quite useless to carry on investigations upon this point, unless those investigations are carried out accurately and minutely, and unless they are carried out definitely upon the measured mile—not upon voyages of ships, which I hold are most delusive things, because however nearly you may suppose the circumstances to accord, we all know very well that, even in fine-weather voyages, there are many things, as a captain of a ship can tell you, which make differences in the voyages of the ship, and we are apt rather to flatter ourselves with favorable results than to test ourselves with crucial experiment. The other point to which Mr. Maginnis refers is the eliminating of certain frictional elements in the working of the propeller by some small mechanical detail. Now, in criticising Mr. Maginnis's paper, I do not rise to criticise the award of the council, because I have not seen all the papers upon the subject; but I only think it is fair to draw the attention of this meeting to the fact that this paper does not, excepting in the most meager way attempt to meet the requirements of the competition.

Mr. **John McFarlane Gray.** Mr. Chairman and gentlemen, I am quite aware the subject of the screw propeller is one which, to all studious engineers, has had great attractions; and I, twenty-three years ago, went into the investigation of the screw propeller with reference to thrust and pitch, and found, as many of us have found, that we are bothered with the following water, and cannot deal with that quantitatively. I unburied some of these investigations in anticipation of this meeting, and read them over again, and I thought I would put them into shape. If for the screw propeller with its inclined blades there could be substituted a complete flat disk of the same circumference, C feet, to be pushed astern by a screwed shaft of pitch P feet, making N revolutions per minute, and if the slip with this disk propulsion be S feet per revolution of the screwed shaft, the thrust would be

$$\frac{64}{32} \cdot \frac{N\,P}{60} \cdot \frac{N\,S}{60} \cdot \frac{C^2}{4\pi} = \frac{N^2\,P\,S\,C^2}{22620}\,.$$

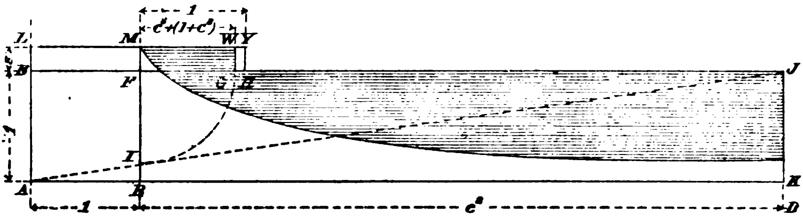
Or, writing

$$s = \frac{S}{P}, \qquad c = \frac{C}{P}, \qquad \text{disk thrust} = \frac{N^2\,P^4}{22620}\,s\,c^2.$$

Applying the same reasoning to a screw propeller, and taking account of the obliquity of the blades, I get

$$\text{Screw thrust} = \frac{N^2\,P^4}{22620}\,s\left\{c^2 + s\,\frac{c^2}{1+c^2} - (1+s)\log_e(1+c^2)\right\}.$$

The part within the brackets, $\{\quad\}$, can be represented graphically thus:



The shaded area multiplied by $\frac{N^2\,P^4}{22620}\,s$ is the thrust in pounds. The diagram is for $c^2 = 6$, $s = .2$. Rankine's expression for thrust, given in the Transactions for 1865, is a more complicated form of the same expression, and by substituting the same letters and rearranging the terms the one can be transformed into the other. My investigation was quite independent of Rankine's, which was seen by me for the first time only this evening. The diagram enables us better to grasp the order in which variations in s, p, c affect the result. I have slightly modified the construction from that first exhibited by me, to clear the diagram of a negative area. If the fractional area W H be neglected, the diagram for a given slip may be extended to the right to represent the thrust factor for any diameter of screw with that slip. The thrust of a screw propeller as compared with the thrust of a plane disk of the same diameter is as the shaded area M W G J K to the area of the rectangle F J D B. The denominator in Rankine's formula comes out to be 22691 in the approximate rule he gives in the Appendix. When the letters used above are substituted that approximation becomes

$$\frac{N^2\,P^4 s}{22691}\,c^2\left(\frac{c-.8}{c}\right)$$

the part $\left(\frac{c-.8}{c}\right)$ being the mean breadth of the shaded portion of the diagram. The approximation is very close between P = .9 D and P = 2.1 D, the usual range nearly. The discrepancy between 22691 and 22620 is due to my using 64 and 32 merely instead of with the decimal place figures. Rankine's approximation is more conveniently applied when written thus. The thrust in pounds is nearly equal to

$$\left(4.465 - \frac{1.137\,P}{D}\right)K\,k\,(D^2 - B^2), \text{ or say} = \frac{36\,D - 9\,P}{8\,D}\,K\,k\,(D+B)(D-B),$$

in which K is propeller speed in knots per hour, k is slip in knots per hour, B is diameter of boss, D is diameter of screw propeller, and P is pitch, all in feet.

Mr. A. C. Kirk. I think it must be a matter of regret that Mr. Gray had not sent in a paper on this subject, but I hope he will supplement what he has said by writing out his remarks more fully.

Mr. J. **FORTESCUE FLANNERY.** Mr. Chairman and gentlemen, I should like to say a word or two, following the same course as Mr. Denny, about the application of this paper to the title which was set forth in the commencement of the competition. No doubt the Council in dealing with the question were obliged to decide on the merits of the papers, not because they exactly followed the lines that were set down by Mr. Carlyle, but because they had to take the paper which was the nearest practical approach to those lines. I am not aware what papers were sent in, nor have I any knowledge of their contents, but it seems to me very likely indeed that the Council were obliged to accept the paper most nearly like the one desired by Mr. Carlyle, or withhold the medal altogether. No doubt that has been their view, and that has been the reason why Mr. Maginnis has had the medal awarded to him. I must say I agree with what Mr. Denny has remarked, and knowing as I do the position which Mr. Maginnis holds, and the facilities he has for obtaining the very best knowledge on this subject, I cannot help saying publicly that I hope he and those who were concerned with him will see fit to allow this paper to be very much elaborated and extended, and to appear upon the Transactions in its more complete form or with an appendix. For example, we are told that these three vessels, of one of the first lines out of Liverpool, had certain results upon their voyages. The particulars of the propellers are given, but no particulars whatever are given in the paper of the machinery or form of hull. Now I think that the relationship of these data is an investigation that could very well be conducted by Mr. Maginnis—in fact I know it is, if he would find time to do it; we should then have some results, of which the paper is at present altogether bare, and not merely statistics, which are no doubt useful, but a complete paper, teaching the lessons which are taught by the performances of these ships. I hope Mr. Maginnis will find it in the interest of this institution, and in his own interest, to make these additions to his paper.

Mr. A. J. **MAGINNIS.** Mr. Chairman and gentlemen, with reference to Mr. Denny's remarks, the reply that I can make as regards the contents of my paper, in connection with the competition for which it has been sent in, is simply this, that I have been in a position fortunately to obtain actual results, and not those of trial trips, which are never afterwards in actual practice borne out, because on trial trips you have always exceptional circumstances—you have picked stokers, you have picked coal, you have experienced men overlooking your machinery, you have your ship trimmed in a proper condition, and in almost all cases you have almost smooth water. I think the statistics given in my tables may be relied upon, being the actual results; for I think I may say positively, take it how you will, or in what way you like, in connection with steamships, if you want to get the true results, and the actual work the steamers are doing, you must get those results from their daily performances. It will not do to take experimental trials for them. Shipowners will not accept from you the result of your trial trips as being what the steamer will do, because in nine cases out of ten—and I think you will all bear me out—afterwards, when the steamer goes on her regular station, she rarely, if ever, comes up to her trial trip performances. Then as regards the statistics which I have given, I have gone into the matter, as much as I know about the propeller, and have endeavored to give them in a way bearing on its efficiency. For instance, from my knowledge and experience of the steamers named by me—*Alpha*, *Beta*, and *Gamma*—extending over a number of years, I have looked over the logs of the voyages, when the larger propellers were on, and have noted the state of the weather, the draught of water on the voyage, and the length of time the ship was out of dry dock. Having got all these particulars, I then looked over the logs of the voyages when the smaller propellers were on, and picked out voyages made under similar circumstances, and have placed them together, so that the relative merits of the propellers may be more easily seen; the voyages marked A, being those of fine-weather passage, and those marked B of a rough-weather passage. I have myself gone over those logs figure by figure, and have endeavored to make the tables as near as possible correct, especially as to the draught of water and the time out of dry dock as these circumstances materially affect the propeller. Of course it is impossible in these results to get the dates within a fortnight or so of each other. As regards what Mr. Flannery said in reference to the extension of my paper, I am very much afraid ship-owners are not inclined to allow such extensive particulars to be made public as he would like me to add. I have purposely omitted all dimensions of the vessels and engines, so as to keep the tables as simple as possible, and because it seems unnecessary to draw up elaborate tables of particulars, which may be found in the underwriter's books. Unfortunately, I have not been permitted to give the real names of the steamers, but if any gentleman should like further information concerning the vessels, I am at liberty to give him the names. I have, throughout the paper, endeavored to show, not my own ideas on the subject, but those which the actual practice of a few years has taught. Most of the facts mentioned in my paper have come directly under my own notice, and I have no hesitation in saying that no author or writer who has ever written on the subject, or ever will write, would think of, or even expect the strange breakdowns which have occurred, or suggest the improvements which actual experience has taught during the past few years.

ON THE RESISTANCE GIVEN TO SCREW SHIPS BY THE ACTION OF THE SCREW PROPELLER, AND HOW TO REMEDY IT.

By ROBERT GRIFFITHS.

[Read at the twentieth session of the Institution of Naval Architects, April 3, 1879, N. BARNABY, C. B., vice-president, in the chair.]

When making a series of experiments with a ship-model of 5 feet long, 10½ inches beam, propelled by two separate springs of equal strength, and representing a screw ship with separate engines, I discovered that there was considerable suction of the eddy-water caused by the screw at the upper part of its disk. I had a telescopic pipe ¾ inch diameter, which led from a tank inside the model, and which could be pushed out till the end was close to the top part of the screw's disk. When this pipe was pushed out and was plugged at the inner end, the model went 48 feet in a minute with 700 revolutions of the screw; again, when the plug was removed from the pipe and the water was allowed to flow into the tank, and be drawn out by the suction of the screw, the model went 61 feet, with 700 revolutions; while, when the pipe was pushed in, the model went 75 feet with 745 revolutions. When the model was moored the water would flow in through the pipe and fill the tank in 10½ seconds; if the screw was then started it would empty the tank in 10 seconds, and when the model was going the screw would empty the tank in 8 seconds.

I will now refer to some trials made on Messrs. Penn & Sons' ship, the *Elephant*, with a four-bladed screw 8 feet diameter and 11 feet pitch, the boss being 4 feet 6 inches diameter; this screw was first tried in a casing, and the speed was 7.836 knots with 239 indicated horse-power; the casing was then removed and the speed was 8.296 with 240 indicated horse-power; the speed, with an ordinary three-bladed screw 8 feet diameter and 10 feet pitch, was 8.416 knots with 238.6 indicated horse-power. The *Elephant* has a very full stern, and I was disappointed with the results until I made further experiments, which proved that with a full-stern vessel a casing over the screw and an enlarged center boss would cause a loss of speed, as the currents from each side of the stern come on to the outside of the casing, and the screw has to get its supply from the dead water in front of it. When the screw is working without a casing it takes a great portion of its supply from outside its disk at the forward edge and as far as the middle of the blades, and draws in the currents towards it, by which it reduces the eddy. With a fine run ship the currents meet nearer the stern and the dead water is much reduced.

The first experiment I made to find out the cause of the unsatisfactory results in the *Elephant* was making the projections on each side of the deadwood which covered the boss longer, and tapered towards the screw instead of being parallel with the screw-shaft; this caused a loss of 10 per cent. in the speed of the model, and gave an idea of what was required. I then shortened the stern and made the run, as high up as the top of the screw, end 2½ inches in front of where it originally ended; from the end of the run to the stern-post I had a parallel piece of the thickness of the stern-post, and the screw was left in the original position; this of course made the run or wedge form of the stern fuller. By this alteration the model gained 15 per cent. in speed, the power exerted being the same. I then altered the model to what it was before it was shortened, which reduced the speed to which it originally was. I have since made experiments with four-bladed screws with blades the same width from the boss to the points, and with blades tapered from the boss to the points on the leading edge only, the result being that by lengthening or fining the run there was a slight increase of speed of 3 to 5 per cent.; but by shortening the run and making it fuller, and placing the screw about two-thirds its diameter behind the tapered part, and leaving the intermediate space open, or filling in a parallel piece of the thickness of the stern-post, an increase of speed of 10 to 15 per cent. was obtained, for which I account in the following manner: As the ship moves forward the water closes in from each side of the stern to fill the space she occupied; when the screw is in operation it sucks away the eddy-water at the top part of its disk, which mostly accumulates there, owing to the fullness of the run from the level of the screw-shaft to the top of the screw, which water would follow the ship if the screw did not draw it away, by which the thrust given to the screw-shaft is increased, and increases the resistance of the ship to nearly twice as much as it would have been had the screw not been in operation. The only remedy that I have been able to discover is to place the screw as far back from the tapered or wedge-part of the stern as will allow the eddy-water to remain there, for which purpose the screw must be placed about two-thirds its diameter behind the stern-post, so that the currents which flow on each side of the deadwood will by drawn by the action of the screw and meet in front of it, and supply it with water to act upon.

Fig. 1.

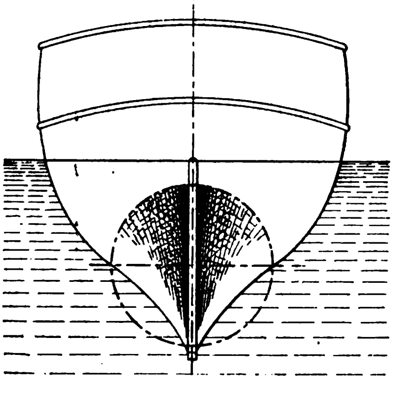
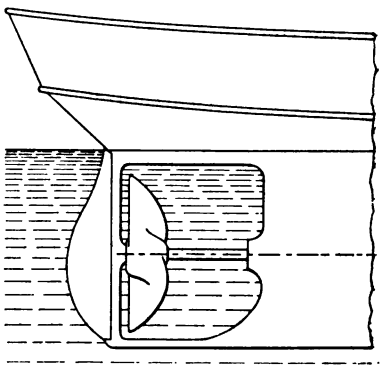


Fig. 2.



In order to obtain the full effect of the power that is exerted to propel the vessel, it will be necessary to form the sterns of screw ships, as shown in Figs. 1 and 2, unless ship-builders can give a valid objection to it; the shortening of the deadwood would reduce the displacement but little, and that could be compensated for by enlarging the counter above the screw, as shown, which would then take a portion of the water which the screw forces back in its wake.

There is one other feature in connection with these experiments which proves the theory of my improvements; a piece of the deadwood 1½ inches long was removed, the screw being left in the usual position; this made no difference in the speed, which was 51 feet in half a minute with 525 revolutions in 60 seconds. The screw was then moved ½ inch forward, when the speed was reduced to 46 feet with 525 revolutions; the screw was then moved another ½ inch forward, and the speed was further reduced to 40 feet with 520 revolutions, and, finally, when the screw was moved a third ½ inch forward, the speed was only 26 feet with 500 revolutions. The screw with the leading part of the blades tapered off was then tried in the first position, and gave a speed of 54 feet with 545 revolutions; when moved ½ inch further back the speed was increased to 58 feet with 550 revolutions, and at an inch back 59 feet with 550 revolutions, and at 1½ inches back to 60 feet with 555 revolutions.

There is very little doubt that in full-stern and other ships the dead-water that is formed between the stern and the place where the outside streams meet goes forward with the ship, and forms the wedge or taper end to the deadwood; this does not cause nearly the resistance to sailing ships that it does to screw ships, through the action of the screw sucking it away. Colonel Beaufoy, by his experiments, upwards of half a century since, found that when the taper or wedge end of his model was at an angle of 6° 22′ 45″ there was no resistance at any speed up to 8 knots, but that when the angle was 19° 28′ 16″ the resistance at 8 knots was 34.34 pounds, and gradually increased from 0.2101 pounds at a speed of 1 foot per second to 34.33 pounds at 13.527 feet per second, his model being 12 inches square, with tapered wedge ends.

Screw propellers, when working, act on the currents of water which flow by the stern of the vessel, and by accelerating the velocity of those currents meet with the resistance by which they are enabled to push the ship forward; there is, however, considerable difference in the velocity at which the currents flow at different parts of the screw's disk, and consequently the screw's blades meet with more resistance in passing over some parts of the disk than others; this of course results in the screw giving greater thrust in some positions, as shown in the dynamometer diagrams of H. M. S. *Rattler*, in which the thrust varied between 2.9 and 4.1 tons in each revolution. Besides indirect proofs from thrust diagrams, direct proof of the difference of the velocities of the currents was obtained from experiments I was allowed to make on H. M. steam pinnace No. 22, at Devonport, in 1875, by measuring, with apparatus specially constructed for the purpose, the rate at which they flowed through the screw's disk while the boat was towed. These experiments showed that over the bottom half of the screw's disk the water was little interfered with, and the water passed through the disk there at nearly the speed at which the boat was towed, but at the top half of the disk the water was dragged with the boat to a certain extent, and only flowed through the disk at about half the speed at which the boat was going. Fig. 1 shows the results, the currents having the least velocity where the shading is greatest.

When a screw is working, the blades as they pass over the top part of the disk meet with more resistance than when they are passing over the bottom part, for as the screw, to obtain the thrust, drives back a column of water the size of the screw's disk at a velocity corresponding to the power employed, it has to accelerate the velocity more at the top of the disk than at the bottom, and consequently more power is expended on the top part of the disk than on the bottom, and from this cause the increase in the resistance of the ship from the action of the screw, which Mr. Froude has proved is always from 40 to 50 per cent. of the total resistance of the ship, is considerably more than if the power were uniformly expended over the disk, for the ship is much retarded by the drawing away of the dead-water (so to speak) from the top of the disk, whereas accelerating the currents at the bottom of the disk would have hardly any effect on the ship. Again, as the blades one after another pass over the top part of the disk, and as each blade meets with an in-

Fig. 3.

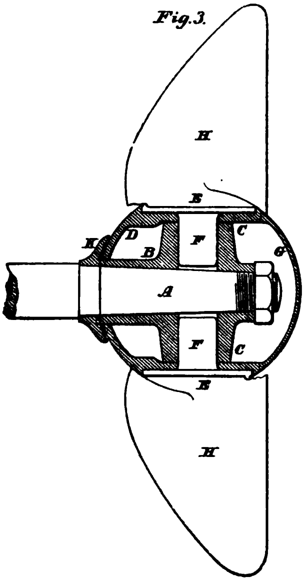


Fig. 5.

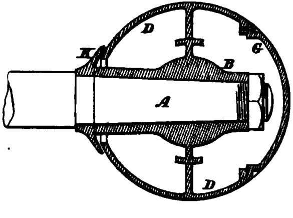
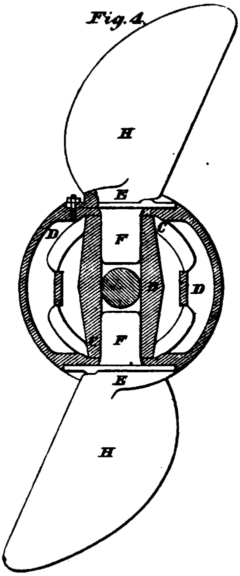


Fig. 4.



crease of resistance there, a series of jerks is given to the stern of the vessel, which is the cause of vibration.

Figs. 3, 4, and 5 show a screw so constructed that the blades always met with the same resistance. On the shaft A is fixed a casting B, with two sockets C C on the opposite sides; the shell D of the boss, which can just pass over these sockets, has an elliptical hole in the forward end, through which the end of the casting B passes, and which allows of a suitable amount of movement. The blades H H, which are formed so that most of their surface is aft of the middle of the boss, are constructed with flanges E E, and shanks F F; these flanges are bolted in the usual way to the shell D of the boss, the shanks slipping into the sockets C C; a cap G completes the shell of the boss, and a cover K fixed on the shaft in front of the boss prevents floating substances entering the boss through the elliptical hole. It is obvious that as the blades have more than half their surfaces aft of the middle of the shanks, the pressure on their surfaces tends to fine their pitch; and also, since they are rigidly connected with the shell D of the boss, that as one blade turns the other is also turned, the pitch of one being increased as that of the other decreases; when, therefore, one blade meets with more resistance than the other, the difference of pressure causes that blade to turn, reducing its pitch and increasing the pitch of the other until the pressure on each is equal. By these means the injurious action of the screw is to a great extent done away with. The cost of construction of this screw would not exceed that of an ordinary screw, and no alteration in the shaft or vessel would be necessary for applying it, and at any time the blades could be fixed and the screw worked as an ordinary screw.

DISCUSSION.

The **Chairman**. I do not know whether Mr. Griffiths' description of his proposed form of screw has been quite understood. There is a model here which is intended to illustrate it, but if it has been sufficiently understood I shall be glad if the gentlemen present will offer any observations they may have to make.

Mr. **Benjamin Martell**. I should like to ask Mr. Griffiths what his experience has been in reference to the trunk that he proposed some time ago, and which, I think, he had fitted to some models, where the screw worked in it, and he said that he found a very great benefit was derived from that. He intended, I understood, to carry out those experiments, and stated in a paper read on it here, either last session or the session before, that he hoped considerable benefit would be derived from that. I am rather disappointed that Mr. Griffiths has not alluded to it in this paper, and I should like very much to know what the result of his experiments has been in regard to that.

Mr. **THORNYCROFT.** Mr. Chairman and gentlemen, I should like to make one or two remarks on the screw propeller, particularly on the very interesting diagram which Mr. Griffiths has given us, where he refers to the difference of speed of the water through the screw disk, at the upper and lower parts of it. I did not quite catch the figures, but there appeared to me to be a very great difference of speed in the water at the disk in the different parts, and the arrangement which Mr. Griffiths has shown us seems certainly calculated to give some advantage; the only doubt in my mind is whether or not the mechanism can be made so that the alteration of form which occurs in the screw shall not be accompanied by too much wear. There is another thing I have thought about, which this screw bears upon, which is this: that in steering a screw vessel I lately came to the conclusion that there was a new element—at least, new to me—in side resistance. Other people may have thought of it, but when a screw is propelling a vessel rapidly, and there is great power being exerted on the screw blades, any force exerted to move the stern of the vessel sideways is opposed by the screw blades working unequally in different parts of their revolution, and so opposing that force. You alter the position of the screw disk where the greatest work is done if you move the vessel's stern sideways, and I have come to the conclusion that there is very great stability in resisting side motion in the action of a screw propeller. Another curious thing which happens is this, that Mr. Griffiths' propeller, if it works well, is calculated to remove this difficulty of steering in a screw vessel. We have lately had our attention called to an American vessel where this difficulty was got over in a curious way, by taking the screw nearer the middle of the vessel, so that it could not stop the vessel turning. I do not know that I have any other remarks to make.

Prof. A. B. W. **KENNEDY.** Mr. Griffiths having alluded to model experiments, I should like to say a word or two on the nature of the experiments, from which we can deduce any results whatever about the screw propeller. I think we may say that there are three methods under which experiments, or what we may call experiments, may be made. We may use the results of ordinary working as have been done by Mr. Maginnis, we may get the results of trial trips, or we may take the results of experiments made with models, and certainly the value of any statements made here, in papers or otherwise, depends very much upon the way in which the results which are stated have been obtained. Now, to take first the trial trips. Of course it is very easy to abuse them, and we all know that trial trips do not always represent the actual doings of the ship afterwards. That, however, is purely a commercial question; it is not a question which concerns the efficiency of any particular propeller at all. The difference between the result on a trial trip and the result of any ordinary voyage, so far as this question of mechanical propulsion is concerned, is simply this: We do really know the results of a trial trip, more or less; we do *not* know the results of the ordinary voyage. I think most marine engineers will bear me out in saying that even a very carefully kept log is a thing which can do no more at the very best than give us something like a rough average of what has been done; but comparing the log of one voyage with the log of another voyage under somewhat similar circumstances, and about the same time of year, and with the same kind of freight, is not making an experiment. It is nothing more than giving us a rough comparison. A trial trip is no doubt carried on with picked men, and under very special conditions. An experiment is always made under special conditions. In such an experiment, we are not only justified in picking our men and choosing our conditions, but it is only by doing so that we can know what is actually done on the trial trip. I claim for trial trips that they do give us some results which we can reasonably trust. Whether they are results which the ship will attain afterwards or not is quite a different question, and one which concerns the owners of the ship chiefly; but as marine engineers and naval architects, here we are concerned with having results stated accurately, and I do maintain that we get this far better from trial trips than by averaging the results of ordinary working. As to the experiments made from models, we know from the results that Mr. Froude has given it is possible to obtain results from model experiments which can be applied to actual working; but we also know just as well as that, from the results of Mr. Froude's experiments, that it is impossible to apply the results obtained from models directly to full-sized ships. You must first of all direct your investigation to the comparison of the relations between the models and the ships, so that it is just as easy to make a wrong experiment with models under certain circumstances as not. It is no use trying experiments with models and saying that they will tell us about ships, because Mr. Froude's experiments tell us about ships, unless we can also say that we have investigated the relations of our model to the actual full-sized work, as Mr. Froude has done through these very many years of actual careful work. I do not think, in saying this, that I have at all misunderstood what Mr. Froude has often said as to the relation between his experiments and actual work. I can only say, as having had something to do with experimental work at various times, that really the difficulty of deducing accurate results—results that one can apply scientifically—from the mass of information you get out of even a well-kept log-book is exceedingly great, and that even in the hands of very careful engineers those results must be taken as not representing very much more than averages.

Mr. C. W. **Merrifield**. If Mr. Kennedy had not done it, I intended to have called attention in much the same language as he has done to the importance of knowing, when you are comparing the performance of models with the performance of full-sized ships, accurately the law which connects the performance of the model with the performance of the ship. That is an absolutely necessary complement to the inference of arriving at the performance of the one from the performance of the other. I was particularly desirous to emphasize this, because mistakes are so often made about it, and, in particular, it has sometimes been said that I was wrong in my difference with Mr. Froude at the time I wrote the report to the British Association of the performance of ships, in that I did not consider that any experiment with models was then of any considerable value at all. Mr. Froude has since removed the difficulties that then lay in our way by trying experiments on full-sized ships and verifying experimentally the law which was then a mere assumption. I think it was first stated by Professor Reech, and I believe was also originally, quite independently, reinvented by Mr. Froude, that in comparing the propulsion of ships and models the speeds of the ships ought to vary as the squares of their lineal dimensions; but at the time I wrote that report this was a mere theoretical belief, and was not experimentally verified. It was not until the *Greyhound* experiments were tried on a large scale by Mr. Froude simultaneously with that splendid series of experiments tried at Chelston Cross that that was even approximately verified; and in attempting to extend that law to other details of propulsion beyond the general sort of average of propulsion which you get on comparing the whole speed of one vessel with the whole speed of another, I think you introduce new terms which may or may not be fitted by the direct formula connecting the average performance of the vessels. I think it is very desirable it should be borne distinctly in mind, in comparing the results of models and experiments, that you require not only to assume the law, but to establish it, and to show how it fits every detail to which it has to be applied.

Mr. WILLIAM DENNY. Had Professor Kennedy not risen to correct the impression which seems to pervade certain minds that the more complicated you can get the conditions of an experiment the more valuable that experiment is, I would have risen to explain it myself, and I regret exceedingly to find that a love of rule of thumb, of what is supposed to be the practical man's condition of mind, is apt to lead away so many gentlemen from what is clearly the practical condition of mind, namely, to eliminate from your experiment every undecided quantity and to place it upon a clear basis. I think it is lamentable that this condition of mind should exist, and I think that the sooner gentlemen come round to see that the really practical men who are dealing with the work of the day are Mr. Froude and the gentlemen representing the admiralty, and those who are carrying out experiments and who have attempted through good report and through evil report to fight this impression of the practical working mind and to eliminate all doubtful facts, especially such facts as occur in the voyage of a ship the better it will be. Professor Kennedy pointed this out so ably that I am perfectly convinced that the body of the members of this Institution have clear enough heads to see the thing. I think we can hardly pass over Mr. Griffiths' paper without expressing our great pleasure in having from a veteran in screw propellers the paper which he has put before us. Any one would be inclined to criticise such a paper liberally and generously as coming from him, and I think he deserves the greatest credit for what he has done. He has come forward and tried to put Mr. Froude's theories into actual fact. No ship-builder has yet tried to do it, and I think we owe Mr. Griffiths an acknowledgment for what he has done and for what he has pointed out. The only thing I should like to ask Mr. Griffiths about is this: In the experiments which I believe he carried out with these models he gives us the speed, he gives us the results; but there is one difficulty, what was the power exerted on the model? Well, as far as I have verified his experiments, I have not been able to find out what that power is. I do not think the paper has distinctly and clearly given us the power that is acting at the other end of the shaft, and therefore it is quite impossible to say what the merit of the propeller is.

Mr. A. C. KIRK. I was very pleased indeed to find Professor Kennedy speaking so strongly and favorably of trial-trip experiments as giving us the clear relation between the power and speed of ships. I have analyzed a great many voyages of steamers of various companies, and I must confess it has resulted in comparatively little information indeed. Now, trial-trip experiments will not certainly give you the absolute speed of a ship at sea. A ship at sea never maintains her trial speed, as I think most of us know, but you get the correct relation between the power and the speed, and the speed at sea is simply a question of the power of continuance of an engine—what power you can keep up at sea—which is a very simple affair. Engines will generally keep up at sea, if decently well managed, three-fourths of what they will do if properly worked upon a trial trip. In other cases, if they are decently managed they will do about two-thirds. Much depends on the qualities of the coals supplied, but it runs between those two figures. As to the relation between the horse-power, and the consumption of coal, and the speed, I have not the slightest faith in logs, but I have very strong faith in well-conducted trial-trip experiments, but, as I have said, they must be properly and well conducted.

Mr. RAVENHILL. I feel I must make a remark or two after what has passed from our friends here to-night from the Clyde. I remember the time when trial trips were almost confined to the neighborhood of London. I believe the only measured mile was the mile on the Thames, and it is gratifying to find that our friends now lay such stress on the experiments made on the measured mile. We have been to-night over the old ground, because there are two sides to the question. If you look at it on the scientific side, there can be no doubt that the measured-mile trips are most valuable. The measured-mile trials as recorded by the admiralty, the trials as recorded by the Peninsular and Oriental Company, and possibly others, are most valuable tables of reference; but, unfortunately, a ship-owner cares but little about them. We have that battle to fight with him. You all know perfectly well that the moment a ship is ready for a trial trip the owner wants to send her out to sea, and to begin to earn some return for the capital which he has invested. Once that is done he only looks to the carrying powers of his ship, and the cost of coal entailed on him to convey so many tons of goods from one port to another. Discussions like this may so far do good as eventually to bring ship-owners to give a little more time to experiments such as we should all desire to see made with every steamship before she permanently takes up her station. If a series of experiments with all the ships that are built could only be carried out for say a year or a year and a half, the amount of information we should in that time collect would be most invaluable data for the future. I do not know how we as an Institution can put that prominently before them, but if it could be devised in any way, it would be, I am sure, attended with most beneficial results to all of us. We evidently have a great deal to learn.

Mr. **Edward James Harland.** My lord and gentlemen, I cannot refrain from giving a decided expression on my part against the practice of short distance trips, because the amount of jockeying and dodging that goes on in running a measured mile has been a greater stumbling-block to progress than anything else. I am perfectly satisfied that more sound constitutions in the structure of marine engines are destroyed and ruined for their lives by measured-mile trips than by fair working at sea, and I have regretted exceedingly sometimes seeing the absurd, I may say ridiculous, amount of indicated horse-power that has been actually, no doubt by figures, given out at the trial trips of some of our magnificent frigates, when I am satisfied it would not be safe ever again to attempt it, for one reason, and that is that such a power is never required to be developed, and next, because the parts of the machinery have become so injured or so strained by the extraordinary development of power in the measured mile, that it would be extremely unwise to attempt it again. Therefore, on behalf of the ship-owners, although Professor Kennedy as much as said, "Let the ship-owners look to their part of the business; as professional men, it is for us to see that we get out certain nominal results," I say it is our business as naval architects to consider the interest of ship-owners, because it is on them we have to rely entirely. If we cannot produce that which is practically of use to the ship-owner, our vocation, or our profession, if you choose to give it that name, is entirely gone. Therefore, on behalf of the ship-owners, I say that trial trips over the measured mile are simple deceptions, and if I were a ship-owner I would not allow anything of the sort, and as ship-builders we allow nothing of the sort. When we make a contract for a ship to be finished by us, our contract is that the ship shall give out that work continuously—one hour, six hours, or sixty hours. The boilers are so arranged that the feed can be on during the whole of the time. Now, we know perfectly well how dodging goes on in the feed in trial trips over a measured mile. We know the accumulation of pressure, and all those series of nonsensical matters which I think, as naval architects and engineers, we ought to be entirely above. If our engines are fit to do their work commercially, for that is the real result of the whole thing, they should be able to do it continuously, and not under circumstances only of special stokers, picked coal, and all sorts of nonsense, which, in the early days of naval architecture, were very fine; but we have now to deal with facts, and in foreign competition all that sort of thing is passed. I would strongly urge that we do not allow ourselves to be any longer bamboozled by anything of the kind. I am extremely glad to find that the admiralty have most wisely, latterly, changed that condition of things, and I am delighted to hear that on a recent occasion of a trial trip it was for six hours. I say that is a very decent time. You can have it in the middle of the day and pick your ground, and if the ship will do it for six hours she will do it for sixty, but as for running a paltry mile, your big ship has hardly time to get under way before she is turned round and the whole thing is over. We have the pockets of the ship-owners to consider; the government is another matter, and I leave it for them to consider; but as far as the mercantile marine is concerned, it is really, after all, what will the ship do for six hours or sixty hours?

Mr. PARKER. Mr. Chairman, I am sorry to see our friends from the Clyde on the left, and our friend, Mr. Harland, on the right, so much disagree about these trial trips. I am also sorry to hear that Mr. Ravenhill thinks the ship-owners do not interest themselves nor care much about trial trips being carried on correctly. It may be interesting to this meeting to know that two large firms in Liverpool have arranged with the captains and engineers of their steamers to make correct progressive trials varying from four to the maximum number of knots the vessels are capable of making while on their voyage to India. I had sent to me yesterday a very nice table, the result of one ship's performance; her length is 361 feet, beam 37 feet, depth 26.3 feet, and she was loaded to a draught of 21 feet 9 inches; the speed, power, and consumption per hour is given, progressing from 5 knots to 13 knots. I have also obtained permission from a very large steamship-owner in London to arrange with his engineers and captains while their vessels are on their voyage to make trials of a similar nature. The result of these trials I shall tabulate, and some day I hope to be able to lay them before this Institution.

Mr. H. S. MACKENZIE. I am a stranger here, but if allowed to speak I should be glad to return my thanks to the gentleman before Mr. Parker for taking the side of the ship-owners. I agree with him in a great many points, and I can simply say that I never go by any trial trips, and the trial trips of vessels in which I am interested have always been when laden. They have to run a certain distance along the coast in the direction in which they have to go when deeply laden. If the owners are there, and find the vessel does the speed which the engineers and builders guaranteed she would do fully laden during five or six hours, we feel perfectly satisfied, and the vessels generally have done what the builders and engineers said they would do.

I rise also to say a few words in confirmation of what Mr. Griffiths has said. I happen to be the largest private experimentalizer in screws in this country. I never had any assistance from government or from any association, and I have spent thousands of pounds in carrying on experiments with screws, and I can quite verify what Mr. Griffiths has said. I have a screw yacht of between 40 and 50 tons, and by simply moving the screw in the way Mr. Griffiths has mentioned, I attained very favorable results. I fancy I read an account of it some six weeks since in the engineering papers, and it tallied exactly with my own experience. The further the screw is moved in a space such as is described in that drawing, the better would be the result. But our large merchant ships or men-of-war would have some difficulty with regard to the construction, because it would weaken the vessel to cut out so much. It simply comes to this, that the further you move the screw from the body of the vessel the better, and the gain of one knot in the yacht to which I have referred was made by moving the screw only 9 inches. It was possible to move it 21 inches, the space in the vessel being very large, owing to her peculiar arrangement. I believe the first gentleman that ever adapted that to screw propulsion was Mr. Erichsen, and although there might be other claimants, yet I think I am right in giving him the credit of it. He drives his screw in halves, and each screw, right and left handed, worked in opposite directions on a concentric shaft. Those experiments were carried out to a result which I believe has been attained in this very yacht which I referred to. But on taking off one of those screws, the right-handed one, leaving the left-handed one on, it was close up to the stern-post. The vessel's speed was 9½ knots, with her engines working at their ordinary revolutions, at about 120, with a pressure of 75 on the boiler—no other pressure, no overworking in the engine-room or anything. That is very fair work, 9½ knots with 120 revolutions; but with a screw 5 feet in diameter and 8 feet pitch, simply moved 9½ inches further away from the body of the vessel, she went 10½ knots as nearly as possible all the way from Falmouth to Dover and back again continuously. I should be very happy to have Mr. Griffiths or any member, when that vessel is in the Thames again, to try that with any form of screw; and I feel confident (it does not matter what form of screw), by moving it in that space, as mentioned by Mr. Griffiths, that result will be gained. Whether it is done by having an open space or by having the screw further away from the vessel, I cannot tell, but it so happens it is an open space in my vessel. I mentioned that merely to encourage Mr. Griffiths in what he is doing. Having carried out Mr Froude's calculation's on a small scale, I have verified them on a large one with a vessel of 50 tons, with a screw 5 feet in diameter, with 8 feet pitch, with a revolution of 120; and I have increased the speed of that vessel nearly one knot by a movement of the propeller of 9 inches further away from the vessel.

Mr. R. GRIFFITHS. The first question that was asked was with reference to the trunk. I tried an experiment on a ship of Mr. Penn's, the *Elephant*—a very full-sterned vessel. I put a screw with a boss nearly two-thirds of its diameter and the same pitch as the old three-bladed common screw in a trunk, and on the stern I fitted parallel cylindrical pieces of the same diameter as the boss, to prevent the water coming against the front of the boss. By this arrangement, to my great disappointment, I lost half a knot. I then had the trunk taken off and left the screw as it was, and the speed was then, within a mere trifle, the same as with Mr. Penn's old screw, the three-bladed one. After that I determined to find out the cause of the loss of speed. I first altered my model to the form of the *Elephant*, and then found that I obtained similar results from it to those I had already obtained from the *Elephant*. I next substituted conical pieces for the cylindrical ones in front of the boss, and this resulted in a loss. This first drew my attention to the fact that whenever a screw is near the taper of the hull of a ship the action of the screw in drawing away the water from the tapered surface increases the resistance of the ship, and in order to test this idea more fully I tried various screws, placing them first near the end of the run and then moving them further away gradually. By this I found that in every case the speed increased as the screw was moved aft, until it was two-thirds of its diameter from the end of the run, and there the speed was a maximum, being about fifteen to twenty per cent. more than when the screw was in the usual position, and that as the screw was moved still further back the speed gradually decreased. These experiments were made with various amounts of power, the maximum being 50 feet pounds per minute, and as the weight of my model was 65 pounds, and the speed then about 120 feet per minute, the power divided by the speed was about one two-hundredth of the weight of the model, which is about the proportion which exists in our large iron-clads. This is so simple that I have no doubt in a very few months it will be proved on a large scale. I am now prepared to put a screw on any suitable ship at my own cost, if the owners will give me half the value of the engine power that would be required to obtain the increase of speed that my improvements will effect.

THE STEAM TRIALS OF H. M. S. IRIS.

By. J. Wright, Esq., *Vice-President.*

[Paper read at the twentieth session of the Institution of Naval Architects, April 4, 1879; the Right Hon. Lord Hampton, G. C. B., D. C. L., president, in the chair.]

In the paper on the comparative efficiency of twin screw propellers in deep-draught ships, read by Mr. White at last year's meeting of the Institution, some particulars were given of the first trials of H. M. dispatch vessel *Iris*, and in the discussion which followed a promise was given to communicate to the Institution the results of some further trials, intended to be made with her, with screw propellers of modified forms and dimensions. I now propose to redeem that promise, so far as I am able to do so.

The trials were not so complete or extensive as it was then hoped that they would have been, for the recasting of the original screw blades and the construction of a pair of new two-bladed screws occupied a considerable length of time, and as the ship had to be completed ready for service by a given date, the proposed series of trials had to be curtailed. However, the results of the trials which were made are interesting and instructive, and will, it is hoped, assist somewhat in throwing light on the hitherto little investigated subject, the application of twin screws to fast steamships.

I have appended to the paper a list of the principal dimensions of the *Iris*, and her engines and boilers, but as the chief subject of this paper is the performance of the screw propellers, I will not take up time by reading these particulars.

The first official trial, made on the 14th December, 1877, was what is known as the contractors' trial, of six hours' duration, at full power, at sea, to test the efficiency of the machinery generally. The following are the particulars of this trial:

Draught of water:	
Forward	15 feet 8 inches.
Aft	20 feet 7 inches.
Area of midship section	702 square feet.
Displacement	3,300 tons.
Mean pressure in cylinders:	
High	41.5 pounds.
Low	11.9 pounds.
Average revolutions per minute	90.318.
Average indicated horse-power	7,060.
Speed of ship, about	16.4 knots.

On this trial and on a preliminary trial made just before, it was seen that the speed of the ship was about a knot an hour below what was expected by her designer, the director of naval construction, whose estimate of the speed was confirmed by the result obtained in Mr. Froude's experiments with her model, and it was determined, as had been done from time to time before, to make several series of progressive measured mile trials, to ascertain her performance correctly, and from the results to try and find out the cause of the deficiency of speed.

I can testify from personal attendance at the trials, that they were all most carefully made by the experienced officers at Portsmouth, but you will, perhaps, find in analyzing the results of the trials at the lowest speeds, that in spite of all the care taken some discrepancies appear. These probably have arisen from the difficulty of obtaining the indicated horse power with sufficient accuracy, and from some variation in the amount of the friction of the engines on the several days of the trials. The two pairs of engines have eight cylinders, four high and four low pressure; at low powers the indicated pressures are very small, and the increments of error which may arise from the friction of the indicators and the measurement of the diagrams, where the top and bottom lines are very close together, will tell much more than at the higher powers.

The hull of the *Iris*, as you are aware, is of Landore steel, and the bottom was coated with Sims' composition. The state of the hull, both above and below water, was, as nearly as possible, the same on all the measured mile trials, and they were all made at about the same draught of water, viz, 15 feet 8 inches forward, and 20 feet 6 inches aft. Fortunately, also, the force of the wind and the state of the sea were so nearly similar on the days of the trials that these influences on speed may be left out of account in considering the results of the trials.

At the above draught of water the midship section is 700 square feet; the displacement, 3,290 tons; and the wetted surface, 18,600 square feet.

The original screws of the *Iris* were four-bladed, as shown in Figs. 1, 4, and 7, Plates VI and VII; diameter, 18 feet 6½ inches; the pitch at the leading edge was 17 feet 2 inches, at the after edge 19 feet 2 inches, and the mean pitch, as measured, 18 feet 2 inches. The disk area of the blades is about the usual proportion for four-bladed screws, viz, three-tenths of the whole disk. This type of screw had given very good results in a number of single-screw ships, and notably in the *Himalaya*, where it gave the best results of any she has had during her long career. The case of the *Iris* shows, however, that a propeller which has proved very efficient for a single-screw ship may not be well suited for a twin-screw ship.

The particulars of the speed trials with the original screws are given in the table under first series, and the indicated horse-power curve is shown on Fig. 10, Plate VIII. It will be observed that the highest speed obtained with 7,503 indicated horse-power was only 16.577 knots, or about a knot below what was expected. These screws will be referred to as No. 1.

The second series of progressive trials was made with the same screws, with two of the blades of each removed, but no alteration was made in the pitch of the blades left in place. This was considered to be the simplest and readiest method of proving, as was supposed, that the blade area of the screws with four blades was largely in excess.

The particulars of these trials are given in the table under second series, and the indicated horse-power curve, Fig. 10, Plate VIII, shows a great improvement in the performance as compared with the previous trials. It was not considered desirable to put a very high power on these two-bladed screws, or to run the engines at more than about 90 revolutions, and, consequently, the highest indicated horse-power developed was 4,368, with 88.89 revolutions, and the speed obtained was 15.726 knots.

Although this series of trials proved that the blade area of the No. 1 screws was much too large, it could not be accepted as proving that two-bladed screws were better than four-bladed, and as it was desired to retain four blades for the working screws, on account of the less amount of vibration, and of the greater uniformity of action to be obtained in rough weather, the original blades were recast by Messrs. Maudslay, and made to the form and dimensions shown in Figs. 2, 5, and 8, Plates VI and VII.

The reduced diameter is 16 feet 3½ inches, or 2 feet 3 inches less than before, but, as will be seen on comparing the forms of the old and new blades, the effective diameter is reduced in a rather larger ratio than the actual diameter. The pitch at the forward edge of the blades is 18 feet 11½ inches, at the after edge 20 feet 11½ inches, and the mean pitch, as measured, is 19 feet 11½ inches, or an increase of 1 foot 9½ inches on the No. 1 screws. The disk area of the blades is .288 of the whole disk. The blades are curved aft a little towards the tips, with the view of keeping the points rather further away from the A brackets, and of checking, in some degree, any centrifugal tendency of the water acted on. The blades were polished on both sides to reduce friction, and the edges were made sharp. The original bosses to which the new blades were attached had each a conical tail-piece added. These screws will be referred to as No. 3.

The particulars of the trials are given in the table under third series, and the indicated horse-power curve is shown in Fig. 10, Plate VIII. This shows an improvement on No 2 screws with two blades, and, as compared with No. 1 screws, an increase at full speed of 2 knots, with only 211 indicated horse-power more. The maximum speed was 18.57 knots, or about a knot more than the original estimated speed. The vibration was very moderate, considering the large amount of power put through the screws, and the fineness of the stern of the ship.

As the full power of the engines could not be exerted on the No. 2 screws with two blades, a pair of full strength two-bladed screws of the usual well-known Griffiths' shape were made. The blades were polished on both sides, like those of No. 3 screws, and the edges were made sharp; the boss was spherical, and had a conical tail-piece. The diameter is 18 feet 1½ inches, the pitch at the leading edge 20 feet 3¼ inches, at the after edge 22 feet 3¼ inches, and the mean pitch as measured 21 feet 3¼ inches. The disk area of the blades is .19 of the whole disk. These screws will be referred to as No. 4. They are shown in Figs. 3, 6, and 9, Plates VI and VII.

The particulars of this series of trials are given in the table under fourth series, and the indicated horse-power curve, the best of all, is shown in Fig. 10, Plate VIII. The highest speed given by these screws was 18.587 knots, with 7,556 indicated horse-power.

The vibration set up in the vessel with these screws in use was greater at all speeds than with either of the four-bladed screws. At the 12½-knots speed the vibration was very moderate, but it increased greatly up to the 15¾-knots speed, and from this it decreased again until the maximum speed of 18½ knots was reached, when it was nothing unusual. After this series of trials was made it was determined to retain the No. 3 screws for use on the vessel, although their performance on the measured mile was not quite so good. It was intended to reduce the diameter and increase the pitch of No. 3 and No. 4 screws, and try them again, but time did not permit of this being done. The mean pitch at which each screw was set was determined by the revolutions the engines were required to make to develop at least the specified indicated horse-power 7,000. The engines being compound, it was necessary to run them at fully 90 revolutions to get them to take enough steam of 60 pounds pressure to produce the specified power.

You will observe that the blades of all the screws were made with a small increase of pitch, 2 feet on the width of the blade, with the view of diminishing shock at the leading edge. All the screws were made of gun-metal, as usual in the navy.

It had been suggested that the engines of the *Iris*, having four high and four low pressure cylinders with the accompanying large number of moving parts, might absorb more power than usual in dead load friction. In order to test this by actual trials, the screw shafting was disconnected at the first coupling, and the engines run at various speeds from 18 up to 90 revolutions per minute. At the low speeds the top and bottom lines of the indicator diagrams, taken with the weakest springs available at the time, were so close together that the mean pressure could not be obtained with sufficient accuracy to warrant me in giving the results as reliable. At the highest speeds, however, I believe the diagrams may be taken as fairly trustworthy.

Each set of engines was kept running steadily while two sets of diagrams were taken. The port engines running at 87.5 revolutions per minute gave 202 indicated horse-power, and the starboard engines at 90.5 revolutions gave 196 indicated horse-power, or together 398 indicated horse-power, in dead load friction. The trials at all speeds showed that the starboard engines worked with the least friction.

In Fig. 11, Plate VIII, are shown the indicated thrust curves for the measured mile trials with the several screws. The indicated thrusts have been calculated by

$$\frac{\text{I. H. P.} \times 33{,}000}{\text{mean pitch of screw} \times \text{revs. per min.}} = \text{Thrust.}$$

The base line shows the speed in knots, and the ordinates the thrust in tons. The curves have been completed in the manner devised by Mr. Froude, and it will be seen that the curve for No. 1 screws shows a dead load friction for the machinery equivalent to an indicated thrust of 5.3 tons.

Now, as before stated, the indicated horse-power of the two sets of engines, running disconnected from the screw shafting, was 398, and this converted into equivalent thrust for No. 1 screws gives 3.78 tons. When this is deducted from the 5.3 tons shown by the curve there is left 1.52 tons as the amount due to the friction of the screw shafting. From this we find that the horse-power to overcome the resistance or initial friction of the screw shafting alone at 90.5 revolutions, would be 170. The aggregate initial friction of the engines alone at 90.5 revolutions absorbs practically 400 horse-power, and the sum for the engines and shafting 400 + 170 = 570 horse-power. The developed indicated horse-power, with the same speed of engines and the original screws, would amount to 7,330, and therefore the power expended on dead load friction was only about 8 per cent. of the gross power. This shows that the engines were so well made and the bearings so well adjusted that they worked with unusually little friction. They compare well in this respect with the machinery of those single screw vessels of the mercantile marine, the progressive trials of which were analyzed by Mr. Froude and explained at the meeting of 1876. In these he found the average value of this loss to be about 13 per cent. of the indicated horse-power, and he assigns this as the probably general value.

I come now to the question why was the performance of No. 1 screws with four blades so much inferior to the performances of all the others?

Authorities on the subject of screw propulsion teach us that the effective thrust or efficiency of a screw propeller depends upon the quantity of water acted on in a given time, and the sternward velocity impressed upon it, and that it is always preferable to make the sternward velocity of the current small, by adopting a form of screw which will act on the largest possible quantities of water. Indeed, it has been said that if it were not for the objectionable element "surface friction," there would theoretically be no objection to an indefinite extension of blade area. In short, it has been always considered as essential to the efficient performance of screws generally, that their blade area should be large, and that the water must be permitted to flow freely to them.

Now, No. 1 screws with four blades *appear* to fulfull the above conditions very completely; for the blade area is undoubtedly very large, and as the run of the *Iris* is very fine, there is no obstruction to an ample supply of water for the screws. Notwithstanding this, however, the results of the trials appear to show that on account of the large diameter of the screws, the shape, and the large area of the blades, combined with a fine pitch and consequent high rotary velocity, an extraordinarily large amount of power was absorbed in the surface friction of the blades. But seeing what a large amount of power appears to have been wasted at the highest speeds with No. 1 screws, it is difficult to believe that it can be all credited to this cause, and it is interesting to form an estimate of the amount absorbed in the surface friction of the blades. In our present state of knowledge it is difficult to determine this with even approximate accuracy, but the elaborate and valuable experiments made by Mr. Froude, in 1872, on the friction of planes, with surfaces of various kinds, traveling edgeways through water, furnish data which at all events enable us to form an estimate of the friction of screw blades under certain conditions and with certain reasonable assumptions. The following are the results of the calculations which have been made:

For Nos. 1 and 2 screws, which were coated with Sims's composition, the co-efficient of friction has been taken as a mean of those given by Mr. Froude for a surface of varnish and one of fine sand. For a mean length of surface of 3½ feet, the width of the blades, this amounts to .00564, and adding 10 per cent. for edge resistance the co-efficient becomes .0062 for these screws.

The polished surfaces of No. 3 screws were probably less smooth than Mr. Froude's tin-foil surface, and about intermediate between it and the varnished surface. For a mean width of blade of 3 feet the co-efficient for this screw with 10 per cent. added as before would be .0038.

No. 4 screws had a similarly smooth surface, but the mean width of blade being 4 feet the co-efficient becomes .00365 inclusive of 10 per cent. for edge resistance.

Time will not admit of the details of the calculations being given, but the results showing the horse-power* expended on blade friction are as follows:

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*It must be remembered that the powers above given as actually expended at the screws on blade friction, are less than the indicated horse-powers which must necessarily be developed at the engines to overcome these resistances. As the proportion in which the indicated horse-power exceeds the power actually expended on friction at the screws is not obtainable, no estimable of it has been attempted in the paper.

No. 1 SCREWS.

	Horse-power.
At 91.04 revolutions per minute	1,120
At 82.148 revolutions per minute	820
At 65.105 revolutions per minute	410
At 43.358 revolutions per minute	120

No. 2 SCREWS.

At 88.89 revolutions per minute	530
At 81.2 revolutions per minute	400
At 65.06 revolutions per minute	210
At 45.07 revolutions per minute	70

No. 3 SCREWS.

At 97.189 revolutions per minute	420
At 85.388 revolutions per minute	280
At 61.343 revolutions per minute	110
At 40.963 revolutions per minute	30

No. 4 SCREWS.

At 93.25 revolutions per minute	330
At 76.93 revolutions per minute	200
At 59.385 revolutions per minute	90
At 39.15 revolutions per minute	30

To admit of the results being more clearly apprehended, curves are given in Fig. 12, Plate VIII, showing the calculated surface friction for the several screws at the various speeds of the progressive trials. The base line gives the revolutions per minute, and the ordinates show the indicated horse-power absorbed in the surface friction of the blades, obtained as explained above.

In Fig. 13, Plate VIII, are drawn the horse-power curves for the trials with the several screws, but in this case the power expended in the friction of the screw-blades, as obtained from the above calculations, has been deducted from the total indicated horse-power as developed.

It will be seen at a glance that the performance of No. 1 screws is still much the worst, and that on the above assumptions respecting surface friction a large amount of power still remains to be accounted for. But it may be said the coefficient of friction taken for these screws was too small. To go to extremes, let us find the coefficient of friction on the supposition that the whole of the difference of the power as expended on the four blades of No. 1 screws, and the two blades of No. 2 screws was absorbed in surface friction, and we shall then find the power remaining available for the propulsion of the ship, and to overcome other resistances and see whether this appears to be enough. The question of augment of resistance will be considered farther on.

The trial of No. 2 screws at the 15.726 knots' speed gives a ready means of making the comparison. The power developed to obtain this speed was 4,370 with 88.9 revolutions, and the power which would be required with No. 1 screws to give the same speed of ship would be 6,080, with 85.75 revolutions.

The blade friction of No. 2 screws with the coefficient of .0062 was shown to be equivalent to 530 horse-power, and similarly with the same coefficient No. 1 screws to give the same speed of ship would absorb 897 horse-power. The latter power divided by the former gives a ratio of 1.689.

If F be taken to represent the horse-power absorbed in the surface friction of No. 2 screws at 15.726 knots' speed of ship, then, whatever the coefficient may be, 1.689 F will represent the horse-power expended on the surface friction of the No. 1 screws at the same speed of ship. If P be taken to represent the horse-power left after the assumed screw-blade friction has been deducted, P will be the same for each screw to produce the same speed of ship.

$$\begin{cases} 6{,}080 - P = 1.689\ F \\ 4{,}370 - P = F \end{cases}$$

$$6{,}080 - P = 1.689\ (4370 - P)$$

$$689\ P = 1{,}300$$

$$P = 1{,}890$$

The relation between the amount of power absorbed by blade friction of No. 1 and No. 2 screws at any speed of ship is represented by the proportion of the intercepted ordinates (*aa'* : *bb'*) of the respective curves shown in Fig. 14, Plate VIII.

In the case of No. 1 screws 6,080 — 1,890 = 4,190 horse-power would thus be absorbed in friction, and in the case of No. 2 screws 4,370 — 1,890 = 2,480 horse-power. This would correspond to a coefficient of friction = .0288, or about 4.65 times that of the co-efficient taken in the first calculations.

It is evident that this latter estimate of the blade friction is far too great, for, as shown by the above calculations, it would absorb all the power developed by the engines except 1,890, and this small amount which is left would have to overcome—

1st. The initial or dead load friction of the machinery.

2d. The working load friction of the machinery and pump resistances.

3d. The slip of the screws.

4th. The resistance of the ship at 15.726 knots' speed.

5th. The augment of resistance due to the action of the screws.

Now, according to Mr. Froude's experiments with the model of the *Iris*, the indicated horse-power necessary to overcome the resistance of the hull alone—*i. e.*, the estimated horse-power—would be 2,160 at 15.726 knots' speed. We see, therefore, that blade friction cannot be taken to account for anything like the whole of the bad performance of the No. 1 screws, and we must look for other sources of loss.

1st. It is probable that the four broad-pointed blades of No. 1 screws were more fruitful in eddy making than the others, and encountered more loss from this cause; then, as the blades followed so closely on each other, taking, as it were, slices of water about 4½ feet thick, they might disturb injuriously the water for each other. That is, each blade probably came in contact with eddies set up by the blade which preceded it, particularly towards the points of the blades, where they were broadest and the velocity greatest. The eddies would, no doubt, be worst where the blades approached the sides of the ship, and the heads of the A brackets which carry the screw bearings.

As the blades of No. 2 screws followed each other at intervals about twice as long, any eddies set up by one blade would probably be left behind before the other could sweep round upon them.

It may be said in objection to this view, then why was the performance of No. 3 screws, which had also four blades, so good? These screws were of less diameter and area, and of rather more pitch; the blades had narrow points, and they did not approach so close to the sides of the ship by 1 foot 1½ inches. The blades did not attain the same width as those of No. 1 screws until a distance of 6 feet 3 inches from the sides of the ship was reached, and such blades would probably suffer much less rotary obstruction from disturbed water.

The second source of loss with the No. 1 screws must be, I think, in the greater augment of resistance with them than with the other screws. The four broad-pointed blades of large area set so nearly square to the line of motion of the ship, following each other so quickly, and approaching so near to the sides of the ship, must interfere, much more than the other screws, with the stream line motions of the water closing in at the stern to balance the head resistance.

Mr. Froude estimates that the disturbance of stream line motions, by the screw in well-formed single-screw ships, is equal to from 40 to 50 per cent. of increase upon the resistance of the ship, when towed at the same speed. With twin screws there is good reason to believe that the augment of resistance has a less percentage than with single screws, but on the other hand, there is the drag of the brackets or struts and the shaft tubes to be overcome, and in the *Iris* this drag is known to be exceptionally great. The run of the *Iris* is, however, very fine (as you will see by the section at the screws, Figs. 1, 2, and 3), and the screws may exercise less than the usual influence on the stream line motions, as compared with most twin-screw ships. Still the amount of influence exercised must in itself be large, and a consideration of this may help us to understand why the original screws of the *Iris* gave so much worse results than the others.

The great amount of attention which has of late years been given to the improvement of the designs of ships and engines has, it is feared, for a time, prevented such full consideration from being given to the improvement and proper adaptation of the screw propeller as such an important subject deserves. It is understood that cases similar to that of the *Iris* have occurred in the mercantile marine, and if those who have had experience of any such cases would lay the particulars of them before the institution, they would help to elucidate a subject on which much has yet to be learned.

HER MAJESTY'S TWIN-SCREW STEEL DISPATCH VESSEL IRIS.

Principal particulars of ship and machinery.

Length of ship	300 feet
Breadth of ship	46 feet 1 inch.
Displacement at trial draught	3,290 tons.
Coal capacity	750 tons.

ENGINES.—The two set of engines are placed in independent engine-rooms, separated by a transverse water-tight bulkhead. Each set consists of four cylinders, two high-pressure, 41 inches in diameter, and two low-pressure, 75 inches diameter. The high-pressure cylinders are bolted to the fronts of the low-pressure cylinders, and are partly recessed into them. The length of stroke is 3 feet. The high-pressure cylinders are fitted with expansion valves on the backs of the slide valves.

An arrangement of change valves and pipes is fitted to the cylinders, by means of which each is put in direct communication with the boilers and condensers. The engines are thus capable of being worked as simple engines with steam of low pressure. A surface condenser constructed of brass is fitted in each engine-room at the backs of the cylinders. The tubes are ⅝ inches in diameter, and the total cooling surface of each is about 7,000 square feet. One vertical air-pump to each set of engines, 43 inches diameter and 21 inches stroke, is driven by a rod off the foremost low-pressure piston of each set, and by bell crank levers.

Two centrifugal pumps, 3 feet 9 inches in diameter, are employed for circulating the water through the condensers. These are capable of pumping 500 tons of water per hour from the ship in the event of a leak.

The crank shafts are of wrought iron, 15½ inches in diameter. The propeller shafting, with the exception of the aftermost lengths, which are of solid wrought iron, are made hollow, of Whitworth's fluid compressed steel.

The diameters of the lengths inside the ship are 14½ inches outside and 8 inches inside; of the lengths in the stern tubes, 16 inches outside and 10 inches inside; of the lengths outside the vessel, 15½ inches outside and 9½ inches inside. The aftermost lengths of wrought iron are 15½ inches diameter. The length of shafting outside the ship is 53 feet 6 inches to the centers of the propeller bosses. Two brackets 20 feet 6 inches apart are fitted on each side of the ship to support the shafts, which are covered with thin plate-iron castings to prevent ropes, &c., from fouling them.

BOILERS.—There are twelve boilers, placed in two boiler-rooms, which are separated by a transverse water tight bulkhead. The boilers face each other, and are fired from middle-line platforms. The shells of the boilers are made of the same material as the hull of the ship, viz, Landore steel.

The following are the dimensions of the boilers: In after boiler-room, six three-furnace boilers, 12 feet 6 inches wide, 13 feet six inches high, 9 feet 7 inches long. In the forward boiler-room, two three-furnace boilers, 12 feet 6 inches wide, 13 feet 3 inches high, 9 feet 7 inches long; and four two-furnace boilers, 12 feet 2 inches diameter, 9 feet 6 inches long. Diameter of the furnaces, 3 feet 1½ inches. Length of fire grates, 7 feet. Total area of grate, 690 square feet. Diameter of boiler tubes, 3¼ inches. Length of boiler tubes, 6 feet 5¼ inches. Total tube surface, 15,900 square feet. Load on safety valves, 65 pounds.

There are two fixed funnels, with ventilating pipes up their centres. The diameter of the forward funnel is 7 feet 8 inches, and of the after funnel 8 feet 4 inches. The height of the funnels above the dead plates of the lower furnaces is 67 feet.

H. M. S. IRIS.—EXPERIMENTAL TRIALS.

	First series.						Second series.			
	1	2	3	4	5	6	1	2	3	4
Date of trial	Feb. 4, 1878.				Feb. 5, 1878.		Feb. 15. 1878.			
Draught of water:										
Forward	15 feet, 7½ inches.						15 feet, 7½ inches.			
Aft	20 feet. 6½ inches.						20 feet, 6½ inches.			
Immersed middle section, square feet	700						700			
Displacement, tons	3. 290						3, 290			
Revolutions per minute	91. 04	82. 148	65. 105	43. 358	24. 25	18. 0	88. 89	81. 175	65. 07	45. 075
Indicated horse-power	75. 03	5, 251	2, 560	755	217	143	4, 368	3, 306	1, 637	571
Speed, knots	16. 577	15. 123	12. 064	8. 187	......	......	15. 726	14. 517	11. 58	7. 95
$\frac{(\text{Speed})^3 \times \text{mid. sec.}}{\text{I. H. P.}}$	425. 0	461. 1	480. 1	506. 6	......	......	623. 3	647. 8	664. 0	616. 0
$\frac{(\text{Speed})^3 \times (\text{Dispt.})^{\frac{2}{3}}}{\text{I. H. P.}}$	134. 3	145. 7	151. 7	160. 1	......	......	197. 0	204. 7	209. 8	194. 6
SCREWS.										
Description	Common with 4 parallel blades.						Same screws as before, with two alternate blades removed from each.			
Diameter	18 feet, 6½ inches						18 feet, 6½ inches.			
Mean pitch	18 feet, 2 inches.						18 feet, 2 inches.			
Slip per cent	1.57neg.	2.69neg.	3.36neg.	5.33neg.	......	......	1. 99	0. 93	1. 41	2. 29
Expanded area of all blades, square feet	194. 4						97. 2			
Disk area of all the blades, square feet	160. 0						80. 0			
$\frac{\text{Disk area.}}{\text{Area of circle swept by tips of blades.}}$	. 296						. 148			

H. M. S. IRIS.—EXPERIMENTAL TRIALS—Continued.

	Third series.					Fourth series.				
	1	2	3	4	5	1	2	3	4	5
Date of trial	July 3. 1878.					Aug. 1, 1878.				
Draught of water:										
Forward	15 feet, 8 inches.					15 feet, 8 inches.				
Aft	20 feet. 6 inches.					20 feet. 6 inches.				
Immersed middle section, square feet	700					700				
Displacement, tons	3, 290					3, 290				
Revolutions per minute	97. 189	85. 388	61. 343	40. 963	21. 6	93. 25	76. 93	59. 385	39. 15	19. 26
Indicated horse-power	7, 714	5, 108	1, 833	606	184	7, 556	3, 958	1, 765	596	163
Speed, knots	18. 573	16. 564	12. 279	7. 797	......	18. 587	15. 746	12. 475	8. 321	......
$\frac{(\text{Speed})^3 \times \text{mid. sec.}}{\text{I. H. P.}}$	581. 4	628. 8	707. 0	547. 5	......	594. 9	690. 5	770. 0	676. 7	......
$\frac{(\text{Speed})^3 \times (\text{Dispt.})^{\frac{2}{3}}}{\text{I. H. P.}}$	183. 7	196. 8	223. 4	173. 0	......	188. 0	218. 2	243. 3	213. 8	......
SCREWS.										
Description	Modified Griffiths, 4 blades.					Modified Griffiths, 2 blades.				
Diameter	16 feet, 3½ inches.					18 feet, 1½ inches.				
Mean pitch	19 feet, 11½ inches.					21 feet, 3¼ inches.				
Slip per cent	2. 97	1. 50	1.63 neg.	3. 36	......	5. 04	2. 49	0. 07	1.25 neg.	......
Expanded area of all blades, square feet	144					112				
Disk area of all the blades, square feet	120. 0					98. 0				
$\frac{\text{Disk area.}}{\text{Area of circle swept by tips of blades.}}$	. 288					. 190				

DISCUSSION.

Mr. W. Denny. My lord, if the opinion which we had last night from a justly renowned ship-builder with regard to the value of measured-mile trials were accepted, I am afraid that this Institution would be obliged to treat this most valuable and excellent paper as a trifling with its feelings, and a contemptuous treatment of its experience and understanding. We would be obliged to ask that all these elaborately delicate and distinctively conducted trials should be replaced by a series of logs of voyages at different speeds and in all kinds of weather. I ask this institution to consider whether the result would be clearer whether it would be better, and whether it would be more advantageous for the advancement of the science of naval architecture. Now, passing over al this, and expressing what is my own frank opinion, it is that I am glad this paper has been read before us, because it marks once more that the admiralty of this country are taking the lead in this question; and no naval architect who has, as I have, a sincere respect and admiration for that great body and its skill, can help feeling glad that they have come forward and produced a paper which, I think, on the special subject of steam trials, has not been equaled in the history of this Institution. I do not think that I overpraise this paper in any way. Looking to the honesty with which these trials have been conducted, and the careful analysis with which Mr. Wright has separated all the results—how he has avoided the temptation of running rashly to the supposition that the friction of the blades alone causes all the difference—how delicately he has divided one point from another—I think we must again congratulate ourselves that such a paper has been read before us, and I think that this paper stands out as proof, if you will look at those curves, that there is no necessity with progressive trials for either picked stokers or picked coal. Both these items can be safely left over as a superfluity, because both are not of any absolute necessity. What curve of the four that are before you was the one that interpreted the difficulty which surrounded the *Iris's* case? It.was the dotted line in black, the curve of the lowest maximum speed, a curve with two blades taken off the original propeller which did not permit the engines to be driven up to their maximum indicated horse-power, and which did not necessitate picked coal or picked stokers. That curve stated the first interpretation of the question, and in the same way properly conducted progressive trials have no need of a high maximum speed for the purpose of interpreting their results. I may state further before you that so far as the trials of my own firm are conducted, they are untarnished in honesty; and I know this, that the trials personally conducted by the majority of the firms on the Clyde are untarnished in honesty, and there is no man who would dare say one word against them. It may astonish gentlemen here to know that one-half of these trials are conducted without the owners or the owners' representatives being there, and we never suppy the information unless the owner has a desire for knowledge sufficient to induce him to ask for it. The information is got for our own private purposes. I now turn to make a few remarks on particular items in Mr. Wright's paper. Mr. Wright has spoken about the small initial friction that existed in the *Iris*, and I think he is quite right in giving all credit to the engineers for the small initial friction, because it is very small—8 per cent. He quotes from Mr. Froude's paper, which dealt with the trials of a ship built by my firm, the *Merkara*, in which the initial friction was 13 per cent. of the maximum indicated horse-power. But the trials of the *Merkara* were exceptional from some cause or other in this initial friction. We measure our initial friction by what practically comes to the same thing as Mr. Wright's measurement in tons, by the mean pressure of steam referred to the large cylinder. That mean pressure, as most of the engineers here must be aware, varies from 25 to 28 pounds. Now we have had, and not only we, but many of our friends have had, initial friction which varied from 1.8 to 2.5 pounds—that is to say, from a percentage slightly below 8 per cent. to a percentage of about 10 per cent., supposing you take the minimum mean pressure at about 25 pounds. But there is another point with regard to this initial friction which has given me great pleasure, and that is the way in which these experiments of Mr. Wright's have confirmed the value of the discovery of Mr. Froude, that the initial friction does exist and can be measured. Mr. Wright's experiments with the engines running disconnected have proved that with a reasonable addition for the shafting friction, the initial friction shown by the curves is a reality. I can add to that proof something further. We have been fortunate in the case of three ships, to have them tried, not only at a light draught, but at a load draught. Now, the result of these trials was this, that within a very small range the initial friction at the load draught corresponded to the initial friction at the light draught, and corresponded so closely that any little difference might be accounted for. This is an additional proof and confirmation of the reality of the great discovery of that ablest member we have, Mr. Froude. Mr. Wright has very properly said that similar cases to the *Iris* have occurred in the mercantile marine; I am sorry they have not occurred with us. I would have been glad of a blunder to have had such a noble success. But a similar case did occur with the trials of the last large Peninsular and Oriental steamer, the *Kaiser-i-Hind.* I see before me gentlemen who took great interest in her trials, and who worked them out and showed all the energy that one could desire in the matter, and I believe it is quite within the bounds of probability that that company may be induced to add, in the spirit of which Mr. Ravenhill speaks of its old trials, a new set of trials, those of the *Kaiser-i-Hind*, to the contributions which it has made already to our knowledge. I am certain that every naval architect here would say to that company that he appreciated the experiments and the analyses which it had made in that case. Gentlemen, I cannot sit down without again saying how glad I feel that the admiralty of this country occupy the position that they always ought to occupy, and which I believe they always will occupy, of being in the forefront of naval architecture.

Mr. HENRY WIMSHURST. My lord, with your permission I should like to state that I designed and built the first two screw steamships. In the first of these, the *Archimedes*, the screw was made too large for its aperture, and I obtained permission to alter it, or to make another screw. I made another screw, smaller in diameter and in length, but of a greater pitch. When tried, this smaller screw gave an increased speed of two knots per hour. The alteration in the screws of the *Iris* is very similar to that which I made in the *Archimedes*, and affords like results. In the early application of the screw, and in many vessels of modern construction, the apertures are not nearly so large as they should be, nor is the screw far enough from the body of the ship, in consequence of which the propeller blade when passing these parts or places, unduly breaks the water or creates a partial vacuum; the following blade on its approach meets the water falling into such space at great velocity, and in its rear leaves a similar condition of things for the next blade; it is this which causes the jumping motion in the stern of screw vessels, and absorbs a considerable part of their engine power. I have always advised that the screw should be kept as clear of the vessel as is possible, and now I have nothing to add, except that I am gratified to find, that what I did and what I advised forty years ago, is beginning to be accepted as sound in practice.

Mr. A. C. **Kirk**. Mr. Chairman, it is with the greatest pleasure that I have listened to this paper. Fortunately for the profession in general, the *Iris*, by a fit of perversity, so to speak, fell at first so far under what was expected of her that these trials were undertaken. Very fortunately Mr. Denny had pointed out the method of progressive trials for investigating such cases; very fortunately, also, Mr. Froude had investigated the resistance of the hull of the *Iris;* and, also, very fortunately, we have Mr. Wright, a gentleman who has made and given us the most complete analysis of these results, and brought out the *Iris* to a triumphant success. Unless I misunderstand the paper, one knot more was got than Mr. Froude's experiments indicated ought to be got. I do not know, of course, under what conditions Mr. Froude's experiments were made. A more full explanation would be very interesting. Most of the paper is so very clearly written that it is beyond criticism or remark. I would, however, come to the question where Mr. Wright speaks of the engine friction. Now the engine friction is deduced in a method pointed out by Mr. Froude as strictly correct, if the data on which you go are to be thoroughly relied on. Unfortunately, the low part of a progressive trial curve is generally the least absolutely correct. In my own case, in analyzing a good many engines, the variation of engine friction shows that while it may be actually the case that the engines do show this actual variation in friction, I must say that the data at that part of the curve are often a little doubtful, and I am not quite prepared to accept a statement that the engines do show quite such a large variation in friction as the curves would indicate. No doubt the largest element of friction is the state of the piston packing. The friction of the bearings, of course, will tell; but I do not attach so much importance to that, because if the bearings of the engine are so tight as to materially increase the friction, the heating of those bearings will very soon draw attention to the act, and it will very soon have to be remedied. Therefore, until excessive friction is clearly proved to exist in an engine, I do not think it is *prima facie* at all probable that the state of the bearings has very much to do with it; but the piston packings admit of being put very tight—obnoxiously tight—and as the steam carries off the heat of friction of the piston, we are never the wiser until sometimes the mischief is done and the cylinder is cut. I once did know an engine where the piston packing was so tightly set up by a very stupid man, who wished to make it very perfect and steam-tight that in less than half an hour the packing ring had so thoroughly cut and fired into the cylinder that the pressure of the steam, aided by the other cylinder, pulled the center out of the piston and left it in bits at the bottom of the cylinder. That is an actual case that occurred, but only once in my knowledge, therefore I give it merely as an illustration. I find, besides deducting the engine friction from the curve, Mr. Wright has tried some very valuable experiments with regard to running the engine disconnected. The horse-power got there was 398. Of course the friction of the engine under these conditions was 100 per cent. of the whole power. To some extent friction is increased when the load on the piston was increased, but to what extent I do not know exactly. The friction of the bearing, no doubt, would increase very nearly as the mean pressure of the piston. The friction, however, of the packing rings, which probably is the largest part of it, would not necessarily increase at that rate. Perhaps it might not increase at all; probably, however, to some extent it would increase, because there is generally an atmosphere of steam behind the packing rings, as is evidenced by the amount of oil and dirt that gets into them. That is one point on which I feel a little inclined to dissent from Mr. Wright's paper in assuming that 398 was the horse-power of the friction under a full load; I fancied it was more. I think a very valuable set of deductions indeed, and a very original one, are those in which Mr. Wright analyzes the waste of the power in the screw propeller, showing very clearly, in fact, that blade friction will by no means account for anything like the power that was wasted there. The screw propeller works in a very confused current, and possibly the larger diameter of screw loses a good deal of power, because at the one side next the vessel it moves in water, and sets in motion water that is more or less moving along with the ship, while at the tips of the blades probably these large screws were passing through water that was flowing past the ship. Now, as the slip was almost nothing, the probability is that the outside edge of the blades here were moving edgewise through the water for a short space, while those on the inside were violently and suddenly moving a lump of water, in both of which operations there is evidently a waste of power, and the smaller screw evaded that difficulty simply by keeping out of the way of these two extreme cases. I happened to see a case that convinced me very strongly that there are those confused currents. In the case of the *Cyclops*, twin-screw ship, the engines were fitted with the usual disconnecting arrangement, whereby the pins in the coupling can be drawn back with screws. In order that that might be done easily, the pins were a little slack; they were rather too much slack, in fact. So long as we were running straight on at full speed, all went quietly and nicely, but as soon as that was over and we began to do circles and put the rudder hard over, it was a great puzzle to know what was going on, and first one engine and then in a short time the other, made a noise uncommonly like riveting going on in the engine-room. Presently I could trace it to this, that the screw that was going sideways into still water—that is to say, if the stern was going to starboard—as the ship went round the starboard screw set up this hammering, and it was just the reverse if the stern went to port. The engines were making, if I recollect rightly, a very small amount of negative slip—not very much. I take it the action was this: as the screw blades got into the outside water flowing past the ship, a portion of each blade got a blow in the back which caused this back wash. I fancy that was the explanation of it. At all events it came to me as a sort of ocular demonstration that there was a very confused current in which the twin-screws were working. I have nothing further to say, except to repeat, what I scarcely need repeat, the high appreciation we have of such a paper as has been given to this Institution, and it will be a lasting memorial of the progress our admiralty friends have made, and of what they have contributed to this Institution and to the scientific world.

Mr. JOHN R. RAVENHILL. This paper has followed one read last year, and discussed, in which the subject of twin-screw propulsion formed a very prominent feature, and I am sure we must feel deeply indebted to Mr. Wright for the great trouble he has taken in laying the several details before us, and adding so very much to the knowledge of various members of this Institution. There can be no doubt that the fact of the *Iris* being unsuccessful in the first instance, to a certain extent, led to a series of trials which, had it not been for that in opportune result, would probably not have been carried out in the way in which they have been. I was myself connected for a long series of years with the admiralty officials in very, very many trials which were made. I have always spoken of the great care taken in conducting them, and I cannot refrain from availing myself of this opportunity of personally expressing that very valuable information is due, and in fact has always been due, to the results obtained by the measured-mile trials conducted under admiralty supervision. As an old friend of Mr. Wright's I congratulate him on the great success of his paper.

Admiral RYDER. My lord, I should like, with the permission of the meeting, to make a very few remarks on a part of this subject which has been very much talked about lately. I have the advantage of knowing Mr. Froude intimately, and I dare say it is known to all of you that he is in a state of very delicate health, and went abroad some months ago, and I believe he is still absent. He went to the Cape. Previous to that I visited him and saw some of his experiments, and he was then engaged on the question of the dimensions of these screw propellers. The question of the friction of the surface of the screw is one which is of very great practical importance to us at sea in our ships, because we have the surface of our screws, by the action or formation of shells upon them, turned from the very smooth surface they have when they leave Mr. Wright's supervision, into particularly rough surfaces, and we find that that roughness detracts from our speed in a most remarkable degree. It is of great importance, and I think you will allow me to make this practical deduction from it—to keep our screw surfaces polished as much as possible. As naval officers, we get copies of your discussions, and it may be advantageous to the younger ones to know that the first thing they should do on coming into harbor, if they cannot raise their screw, is to send down a diver to clean the surface of it if it requires cleaning. It takes an enormous amount of our speed out of us if we have a very rough screw. I may, perhaps, speaking of the question of surface friction, allude to what Mr. Froude also drew my especial attention to, that whereas we like the whole of the bottom of the ship smooth, it is much more important that the bows of the ship should be smooth than the part abaft the midship section. If naval officers did not know how long they were to be in harbor, their first business would be to polish their screws, then their bows, and after that they might leave the rest of the ship, not paying any particular attention to it.

Captain Colomb. My lord, I should like to add one word to what Admiral Ryder has said. He has spoken of the probability of doing these things—of cleaning the surface of the screw so as to get rid of the surface friction. Now, in the ship that the gallant admiral's flag flew in, in China, we suffered very much from the incrustation of the screw, and also of the surface of the bow of the ship, and it became after a little time our constant habit to do as the gallant admiral has suggested—that is to say, to scrape and polish by means of divers the bows of the ship if we had time, but certainly the surface of the screw itself, and we always found that we gained very considerably in economy of coal by that process. It was not a long job to clean and scrape the surface of the screw, but we always found that we got a good result in economy of coal afterwards.

The President. I am sure I shall only speak the general sense of the meeting if, before I ask Mr. Wright to reply upon the observations we have heard, I tender to him our thanks for a paper of great ability and very great interest.

Mr. J. Wright. My lord, with your permission I should like to thank the gentlemen who have noticed the paper in the complimentary terms in which they have done. I should like especially to thank Mr. Denny, who has done so much in the same line himself. Any expression of approval from Mr. Denny I can only say I highly appreciate. With reference to the question of measured mile trials, generally these trials were made with picked coal and picked stokers. In the case of the *Iris* the picked coal was not necessary, but the picked stokers were necessary, and for this reason: It was desired to keep the steam uniformly at the same pressure at each of the different speeds, and this is better done with picked stokers than ordinary ones. We stationed a trained man at the steam gauge, in the stoke hold, so that the least variation, even of a pound, was seen, and he directed the stokers to manage the dampers so as to keep the steam uniformly at the same pressure. With regard to the question of jockeying at measured-mile trials, I think those who have attended the admiralty trials must know that that is obsolete. With regard to jockeying the feed, every one knows that with surface condensers, such as we have, if they tried it, they would very soon come to grief. With reference to the engine friction, I can only say that I agree with Mr. Denny and Mr. Kirk, that the greater proportion of the variation is in the piston friction, and probably also a little in the slide valves, and in the stuffing boxes. Of course, when the main bearings are well adjusted, any unusual friction will very soon show itself. I am pleased to find that the *Iris* worked with so little friction. Mr. Kirk has asked why we got more speed with the *Iris* than we expected. At the time the experiments were made, we did not very well know what percentage of the gross horse-power the effective horse-power would be. It turned out that she has a larger effective horse-power compared to gross horse-power than was expected, therefore we got a better result. With reference to Admiral Ryder's remarks about the surface friction of the screws, the experiment of polishing the blades of No. 3 and 4 screws did not prove to be of what I should call permanent value; because when the blades of No. 3 were removed, after having been in the water for about ten days, the surface appeared as if it had been covered with finely-powdered lime hardened, and when you put your hand over it, it felt as if you were rubbing your hand on fine sand-paper. Then with regard to the blades of No. 4 screw, they had only been in the water about three or four days, when they began to exhibit the same appearance and to give the same rough feeling to the touch. I think the best thing to do is to get the surface tolerably smooth to commence with, to clean the screws periodically, and when the vessel is docked, coat them with a good composition.

ON THE MINIMUM AREA OF BLADE IN A SCREW PROPELLER NECESSARY TO FORM A COMPLETE COLUMN.*

BY JAMES H. COTTERILL, M. A., F. R. S.

[Read at the twentieth session of the Institution of Naval Architects, April 4, 1879, the Right Hon. Lord HAMPTON, G. C. B., D. C. L., president, in the chair.]

In a paper read before this Institution in 1865, the late Professor Rankine gave a complete theory of the screw propeller, based upon the supposition that the form and dimensions of the blades of the screw were such as to throw back a complete column of water. The distinguished author believed that such was actually the case in well-proportioned screws, and no doubt, the fact that, in many cases, the thrust depends not, to any great extent, on the actual area and form of the blades, but mainly on the disk area, lends strong support to the supposition.

The verification, however, of the theory in the case of the *Warrior* contained in the paper in question, can hardly be considered satisfactory, since it involves the difficult question of the augmentation of the resistance of the ship due to the action of the screw, and, besides, makes assumptions as to the motion of the water at the stern of the ship which subsequent investigation has not confirmed. Experiments made since 1865—I refer especially to those of Mr. Isherwood—had, moreover, led me to believe that the column of water thrown back by a screw is rarely complete, and it was, therefore, with great pleasure that I found that, in a paper read last year, Mr. Froude had made a departure in the subject by comparing the action of a screw to that of a plate advancing obliquely through the water. The theory of Rankine also compares the screw to an oblique acting paddle, but, instead of making use of our independent knowledge of the pressure on the blade, it proceeds by considering, *a priori*, what the change of momentum and quantity of water acted on must be, and fails if that water does not occupy the whole disk area.

Mr. Froude's theory of a revolving plate, if applied to actual screws without any qualification, would lead to the conclusion that the thrust of a screw of given speed and slip might be increased indefinitely, either by diminishing the pitch or by increasing the blade area, results which cannot be accepted. It becomes, therefore, an important question to know what are the limits within which either theory is applicable, and to which theory the action of an ordinary screw propeller most closely approaches. Without attempting a complete investigation, it is in the hope of throwing some light on this question that the present paper has been written.

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* The column is, for brevity, throughout this paper said to be "complete" when all the water passing through the space swept out by any element of the blade is thrown back with some velocity.

In passing from the simple plate to the screw blade, we encounter three distinct difficulties: (1) The change from motion in a straight line to motion in a curve; (2) the change from a flat surface to a curved surface; (3) the limitation of the quantity of water acted on.

In the straight line motion the water is disturbed at some distance ahead of the plate, whereas when the direction of motion is constantly changing, this can hardly be conceived to be the case. Hence the disturbance produced by a plate which moves in a curve resembles that caused by a body when first set in motion through water, and the pressure on the plate must be altered (possibly increased) by the curvature of the path. It is probable, however, that the difference is not important, at any rate for small values of the angle (φ) between the plate and the direction of motion, since otherwise the thrust of a screw of given total blade area would depend on the number of blades to a much greater extent than appears to be actually the case when all other things are equal.

Under the same circumstances the curvature of the plate also cannot much affect the pressure. For let us suppose the plate of very small depth perpendicular to the plane in which the center moves, and let us imagine that plane folded round in a cylinder of given radius, so that the center line of the rectangle becomes a spiral traced on the cylinder, while the plate itself becomes an element of a screw blade of given pitch. The center of the plate then traces out a spiral path on the cylinder, which will be inclined at the small angle (φ) to the element of the blade. The paths of the particles of water relatively to the element will be similar spirals when undisturbed; while near the element they will deviate from such spirals, nearly in the same manner, and to the same degree, as the plane paths in the plane motion deviate from straight lines. Hence the quantity of water acted on, the change of momentum of that water, and consequently the pressure on the element must be nearly the same in the two cases. A complete screw blade can now be built up from the spiral elements, and the thrust determined by summation.

The third difficulty, that the quantity of water acted on by the screw is not unlimited as in the plane motion, but cannot exceed that which passes through the screw disk, is much more serious than the other two, and compels us to restrict the theory to screw blades of small dimensions as compared with the diameter. For in order that the plane motion may exactly correspond to the spiral motion considered above, we must consider, not a single plate, but an infinite number of plates, ranged along a line inclined to the plate at an angle (θ) equal to the pitch angle of the spiral motion, and placed at a distance apart equal to the circumference of the cylinder, if the screw have but one blade, or at the distance of consecutive blades when there are more than one. The pressure on each plate will now be less than that on the single plate, because part of the water which in the single plate is disturbed by its action, is now intercepted by the plates which lie in front of it; and thus it appears to me that the pressure on a screw blade must be less than that given by Mr. Froude's method, because part of the water which would otherwise strike the blade, and which is necessary to develop its full thrust considered as a revolving plate, is intercepted by the blade which lies immediately in front, or if there be but one blade, by that blade itself in the course of the revolution immediately preceding. The magnitude of the deduction will be greater the larger the blades, and some idea of the size at which the deduction becomes sensible may be gained by the following rough method.

Let l be the length of the plate, V its velocity in feet per second, then the pressure on it per foot depth is

$$P = 1.7\, l \sin \varphi\, V^2.$$

Now imagine each particle of water to receive the same change of motion V sin (φ), perpendicular to the plate which the particles in contact with the plate actually do receive, and let b_0 be the breadth perpendicular to the plate which those particles would then occupy, or, as we may conveniently call it, the "breadth of uniform disturbance"; then, disregarding the angle (φ) as always small compared with (θ),

$$Q = b_0\, V$$

is the quantity of water acted on per second, while the pressure on the plate becomes, with feet and seconds as units,

$$P = 2\, b_0.\, V^2 \sin \varphi, \text{ nearly};$$

comparing which with the former value of P,

$$b_0 = .85\, l,$$

showing that the breadth of uniform disturbance is .85 the length of the plate. The actual breadth of disturbance is of course much greater, and hence it follows at once that if the perpendicular distance of two plates be equal to .85 l, a good deal of the water which would otherwise strike the rear plate, will be intercepted by the front one, and the pressure on each one of the row of plates, will be sensibly less than is given by the formula.

To transfer this to the case of a screw, let b be the aggregate length of the circular arcs formed by the projection of the elements of given radius, of all the blades on a thwartship plane, and let C be the whole circumference, then C corresponds to the distance between the plates measured along the line on which they are ranged, while for l we must take b sec θ. Hence we infer that the blades will considerably interfere with each other if

$$C \sin \theta = .85\, b \,.\, \sec \theta\,;$$

or,

$$b = 1.18\, C \sin \theta \cos \theta.$$

In order, therefore, that the theory of a revolving plate may be applicable without sensible deduction, the fraction of the circumference occupied by the blades must not be greater than 1.18 sin θ cos θ, or more probably considerably less. This explains why Mr. Froude's formula for the thrust of a revolving plate gives an infinite result, when the pitch, and therefore the pitch angle θ, is diminished.

And if we increase sufficiently the fraction of the circumference occupied by the elements, I think it is clear that the resulting motion of the water must be nearly such as is supposed in the earlier theory of Rankine. For if the row of plates supposed above are placed near enough together, all the water will move with the same velocity after passing through; that is to say, in the case of the screw elements, a hollow cylindrical race will be generated, the velocity of which will be found, as Rankine finds it, by supposing the velocity parallel to the element unaltered, and the velocity perpendicular to it destroyed. It is true that if we followed the water which strikes any element, we should find that some of it passes on the leading side, and some on the followlowing side of the element, and that therefore the velocity of some particles of water, parallel to the element, is nearly reversed, while that of others is increased. This has considerable influence on the frictional resistance (see Appendix), but since the momentum parallel to the element cannot be altered, the change of momentum must be the same as Rankine supposes, and hence the thrust must be nearly the same. I see, therefore, no reason to alter the opinion I have elsewhere expressed,* that this theory, though not mathematically exact, is very approximately so when a complete column is formed, and I think it applies to this extreme case, with about the same accuracy that the the theory of a revolving plate applies to the other extreme case, where the screw blades are supposed very small.

From what has been said it appears that the thrust of a screw will be less than that given by Mr. Froude's method, by an amount which will be greater and greater as the column approaches completeness, and at length, when the column is completely formed, becomes that given by Professor Rankine's method, after which it can increase no further. If, then, we equate the values of the thrust of a spiral element furnished by the two theories, we shall find the minimum length of the element, which will be capable of generating a complete column. This length is expressed by a fraction, which gives either the fraction of the pitch occupied by a projection of the element on a longitudinal plane, or the fraction of the circumference occupied by a projection on a thwartship plane. In the Appendix it is shown that this fraction is given by the formula,

$$\text{Fraction of pitch or circumference} = 1.18\frac{(q^2+1-s)^2}{q^2+(1-s)^2}\cdot\frac{q}{(1+q^2)^2},$$

where q is the ratio of the circumference to the pitch, and s is the slip expressed as a fraction of the speed of the screw. For reasons which I need not stop to point out, when $s = 0$ we get the formula given above for the value of the fraction at which the blades interfere with each other considerably.

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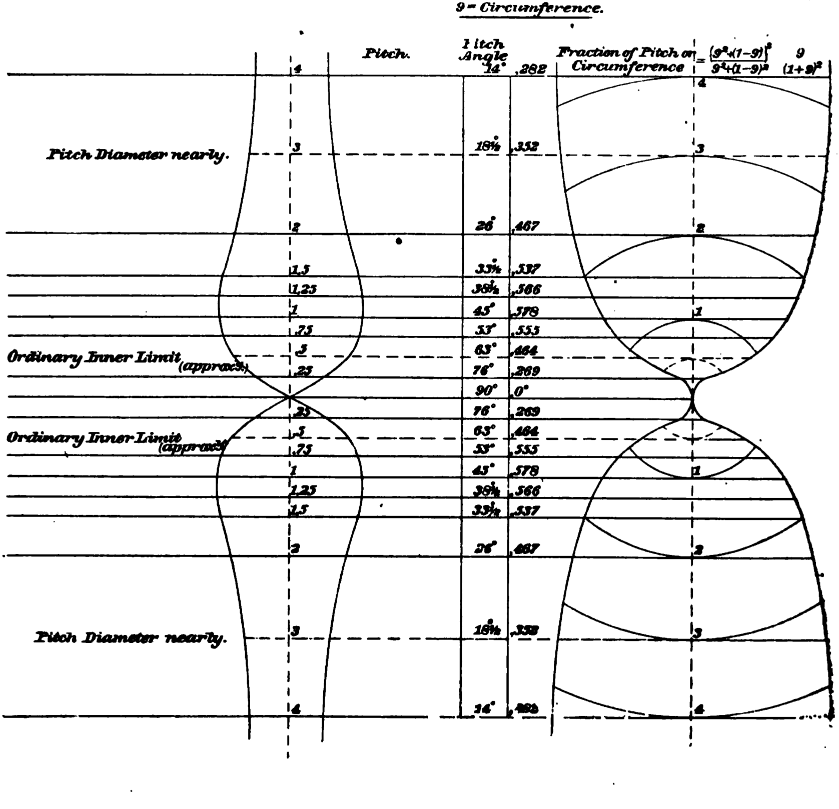
* See "Annual" of the Royal School of Naval Architecture for 1873.

Numerical calculation from this formula, for various values of q, gives the value of the fraction for each element of the blade, and the results may conveniently be exhibited graphically, the several lengths and arcs, when set out at the corresponding radii, forming the longitudinal and thwartship projections of a screw blade, which everywhere satisfies the condition in question. The diagram shown is adapted to the case where the slip is 25 per cent. and the pitch uniform, but the results are nearly the same for all slips between zero and 50 per cent. The numbers given on the diagram are the values of the fraction found from the formula, and refer to the aggregate length of the blades, which theoretically may be divided between any number of blades arranged in any manner: In the diagram the screw is two-bladed, and the values of q range from 0 to 4. In practice, q would not be much greater than 3, or less than .75, limits which are marked on the diagram, and which show the part of the ideal surface, which corresponds to an actual screw blade of pitch about equal to diameter.

We have thus determined the form and dimensions of the smallest screw blade which could, under any circumstances, form a complete column, but it does not follow that such a blade actually would do so; it is more probable, from what has been said, that the completeness of the column increases but slowly with the size of the blade, as it approaches the limit. It appears to me, however, that the diagram fur-



nishes a sort of rough indication of the degree of completeness in which the column will be formed by each portion of an actual screw blade, and hence of the degree the action of the actual screw approaches either of the two extreme cases considered above.

On examination of the diagram, it will be seen that the form is intermediate between the common and the Griffith screw, but that the dimensions are much greater. Like the Griffith, the length of blade is a maximum for a pitch angle of about 45°, but the aggregate length is then nearly .6 of the pitch, or fully 50 per cent. greater than that of the actual screw. On the other hand, the aggregate length at the tips of a screw, the pitch of which is equal to the diameter, is shown on the diagram to be .35, which differs little from that of a common screw.

It thus appears that ordinary screw propellers do not form a complete column, and that the only case in which they approach doing so is in the outer part of a common screw; and as there is reason to believe that the efficiency of a screw is increased by diminishing its length at the tips, it is probable that too great an approach to completeness is not favorable to efficiency; the reason of which may be conjectured to be that, in that case, increase of the blade area increases the thrust much less than it increases the frictional resistances. Another cause, however, may, and, I believe, often does, diminish efficiency when the column approaches completeness, and that is the augmentation of resistance of the ship, due to the action of the screw, which may easily be imagined to increase as fast as the thrust, when too much water is required to supply the screw.

It will now be seen that in actual screws the thrust given by Mr. Froude's method must be much less than that given by Professor Rankine's, while the actual thrust is probably less than either; the deficiency in the second case for any part of the screw surface, depending as it does on the completeness of the column, will be roughly indicated by the approach of the length of that part to the length of the corresponding part of the blade of minimum area. Considered as a method of determining the thrust approximately, it is evident that Mr. Froude's process is to be preferred, and its errors may in many cases be small. It would be foreign to the purpose of this paper to enter on the question of frictional resistances.

In writing this paper I have purposely confined myself to theoretical reasoning, and have not attempted experimental verification. Hardly any experiments have hitherto been published which can usefully be compared with theory in the present state of our knowledge, for either they involve the disturbance of the water by the passage of the ship, or they are made on screws stationary in the water in which they rotate, in which case the conditions are wholly different. Neither of these more complex cases are likely to be successfully dealt with till the more simple case where the screw acts on undisturbed water has been more thoroughly studied.

APPENDIX.

NOTE I.—*Comparison of thrusts.*

Let θ be the pitch angle, p the pitch, and t the (small) depth of a spiral element of a screw blade, r the radius of the cylinder on which it lies, q the ratio of the circumference to the pitch, then

$$q = \cot\theta = \frac{2\pi r}{p},$$

The thrust by Rankine's theory will be

$$T_1 = 2\pi m r t v (v - V),$$

where V is the speed of the ship, v the sternward velocity of the water after leaving the screw, m the mass of a cubic foot of water. Now if v' be the speed of the screw, s the slip expressed as a fraction of that speed,

$$v = v'(1 - s . \sin^2\theta);$$

$$\therefore T_1 = 2\pi m r t . s . v'^2 (1 - s . \sin^2\theta) . \cos^2\theta,$$

which, by expressing $\sin^2\theta$ and $\cos^2\theta$ in terms of q, becomes

$$T_1 = 2\pi m r t . s v'^2 . \frac{q^2(q^2 + 1 - s)}{(1 + q^2)^2},$$

Again, let b be the length of the arc, formed by a projection of the spiral element on a thwartship plane, or if there be more blades than one, the aggregate length of all such arcs, P the normal pressure, and T_2 the thrust of the element considered as a revolving plate; then

$$T_2 = P . \frac{\text{area of projection}}{\text{area of element}} = 1 . 7 b t \sin\phi V^2,$$

where V is the velocity of the element in its spiral path, ϕ the angle that path makes with the element. Now, ϕ is given by the formula,

$$\tan\phi = \frac{s \tan\theta}{1 + (1 - s)\tan^2\theta} = \frac{s q}{q^2 + 1 - s},$$

which shows that for moderate slips ϕ is always a very small angle, its maximum being for $q = \sqrt{1 - s}$, when

$$\tan\phi = \frac{s}{2\sqrt{1 - s}}.$$

For example, for a slip of 25 per cent. ϕ is less than 9°.
Hence we may safely write $\tan\phi$ for $\sin\phi$, and further

$$V^2 = V^2 + v'^2 . \cot^2\theta = v'^2 [q^2 + (1 - s)^2],$$

whence by substitution for $\sin\phi$ and V^2, we find

$$T_2 = 1 . 7 b t s q v'^2 . \frac{q^2 + (1 - s)^2}{q^2 + 1 - s},$$

a formula which gives the thrust of the element in terms of the same quantities as before.

Equate now the values given by the two theories, remembering that in the second the units are feet and seconds, and that therefore we must write $m = 2$ in the first result; then we find

$$b \, . \, \frac{q^2 + (1-s)^2}{q^2+1-s} = {}^{2}\pi \; r \, . \, \frac{q^2+1-s)}{(1+q^2)^2} \, q,$$

which may be written

$$\frac{b}{2\pi r} = 1 \, . \, 18 \, . \, \frac{(q^2+1-s)^2}{q^2+(1-s)^2} \, . \, \frac{q}{(1+q^2)^2}$$

which is the value of the fraction of the circumference, occupied by the elements, when the thrusts are equal.

When $s = 0$ we get

$$\frac{b}{2\pi r} = 1 \, . \, 18 \, . \, \frac{q}{1+q^2} = 1 \, . \, 18 \sin\theta \cos\theta.$$

Now, in the text this same result was obtained on the supposition of uniform disturbance just extending from one blade to the next, and disregarding . The process in this note is the equivalent calculation when ϕ is not disregarded, that is, when the slip is not neglected.

The following table shows the values of the fraction, or what is the same thing, the aggregate lengths of blades of pitch unity, when the condition is satisfied at all points of the blade for a slip of 25 per cent. Variation in the slip alters the result very little except for smaller values of cot θ than can be used practically:

SCREW BLADE OF MINIMUM AREA.

θ.	$q = \cot\theta$.	Fraction.	θ.	$q = \cot\theta$.	Fraction.
14°	4	.282	45°40′	.988	.59
18½°	3	.352	53°	.75	.555
26°	2	.467	63°	.5	.464
33½°	1.5	.537	76°	.25	.269
38½°	1.25	.566	90°	.0	.0
45°	1	.578			

NOTE II.—*On the coefficient of friction of the front of a screw blade.*

The motion of water in two dimensions past a plate set obliquely in the current has been investigated by Lord Rayleigh by aid of a method invented by Kirchhoff. The solution involves infinite surfaces of separation, inclosing an infinite mass of dead water in the rear of the plate, and this is the only way in which motion can take place without loss of head. In fact, however, the surfaces of separation are finite, and inclose eddying water at the rear of the plate, in consequence of which, loss of head takes place. Yet there is reason to believe that in these cases the stream line motion in front of the body does not differ much from the ideal case, and hence Lord Rayleigh's results may probably be applied without serious error.

Now this solution shows that the water passing a plate A B divides into two streams, one of which passes on the side A, and the other on the side B; the point K on the plate where the streams divide being given by the equations

$$AK = \frac{2 + 4 \, . \cos\phi - 2\cos^3\phi + (\pi - \phi)\sin\phi}{4 + \pi \, . \sin\phi} \, . \, AB,$$

$$BK = \frac{2 - 4 \, . \cos\phi + 2 \, . \cos^3\phi + \phi \, . \sin\phi}{4 + \pi \, . \sin\phi} \, . \, AB,$$

where ϕ is the (acute) angle which the plate makes with the direction of the current.

At K the water is at rest, while at points between A and K, or B and K, it moves towards A or B respectively, with a variable velocity which we call v. The friction on the plate is the difference between the friction on A K, and the friction on B K: for example, in the extreme case where the water strikes the plate perpendicularly, the friction is zero.

Thus if f be the ordinary coefficient of friction for a surface F, the total friction,

$$F = f . \int_0^{AK} v^2 . dx - f . \int_0^{BK} v^2 . dx,$$

dx being an element of length of the plate.

Evaluating the integrals by use of Lord Rayleigh's results, we find

$$F = f . V^2 . \frac{4 . \cos\phi - (\pi - 2\phi) \sin\phi}{4 + \pi . \sin\phi} . AB,$$

where V is the velocity of the stream, and therefore the value of the virtual coefficient of friction f' is

$$f' = f . \frac{4 . \cos\phi - (\pi - 2\phi) \sin\phi}{4 + \pi . \sin\phi} = kf.$$

The numerical value of k is found to be for

$\phi = 0$,	10°,	20°,	30°,	90°,
$k = 1$,	.760,	.576,	.434,	0,

showing a rapid diminution as ϕ increases.

Mr. Froude has pointed out that the friction at the back of the blade likewise diminishes when the angle exceeds a certain limit, and states that his experiments show a great diminution in the friction as the angle increases.

DISCUSSION.

Mr. W. H. White said: Mr. Cotterill has given perhaps more attention to mathematical investigation of the theory of screw propellers and their efficiency than almost any one except Mr. Froude and Professor Rankine; and in this paper, as I understand it, he takes the two investigations which are due to Professor Rankine and Mr. Froude, and shows them to be extreme cases; and he attempts an investigation which really connects these two methods, and shows how they are related to one another. I have been looking for Mr. Cotterill's paper with the greatest interest, and I am sure that I only express the feeling of many gentlemen present in saying that it is a paper to be read quietly and studied in comparison with those papers of Professor Rankine and Mr. Froude to which Professor Cotterill has alluded. I am sure that Professor Cotterill would not put forward anything here which does not deserve the most careful attention we can give it, and I for one shall take the earliest opportunity of making a study of the paper.

Mr. J. MacFarlane Gray. Mr. White has expressed exactly my opinion in this paper. This paper is not in a suitable form for reading here.

I think there really ought to be some arrangement to make our meetings more useful, because the time has been almost wasted with regard to most of us in listening to this, no doubt, excellent paper. Those who know something of the subject to be laid on the table should have sent to them before the meeting takes place, some outline of what the paper is to be, as they do in the Civil Engineers' Institution, and then the discussion would be a great deal more valuable. I hope that the remarks I am making may have some influence with the council, and induce them to make some arrangements of the sort which I have mentioned, so that we may know something as to what the paper is to be about.

Mr. J. WRIGHT. I merely wish to remark that I think it would not be doing justice to the valuable matter in the paper Professor Cotterill has given us if we were to attempt to discuss it now. As Mr. White says, it is a paper which requires to be studied. If these papers could be put in the hands of members some time before the meeting, as Mr. MacFarlane Gray has said, it would no doubt be a great advantage. I think it is not altogether the fault of the council that this is not done, because, if the papers were sent in sooner, they could be printed and distributed before the meeting takes place. Perhaps arrangements can be made in future years to admit of this being carried out.

Mr. J. SCOTT RUSSELL. My lord, I quite agree that this is a valuable paper, and I quite agree on the impossibility of giving it adequate discussion on so short a notice. Permit me to observe that a great attempt was made this year by the council and the executive to get in the papers as early as possible. The difficulties in the way of that are serious, because the members have difficult calculations to make, and do not like to send them in until they are completed; but I think the advantages we have had this year, much more than in any other years, of getting these papers printed now, and the prospect we have of getting our volume immediately, are so great, that I think we do well to urge on the council and the executive to take every means of getting the papers printed, if possible, and put into our hands some time before the meeting, because it is quite evident that we come here to give each other the benefit of our opinions upon those particular subjects, and we do not like to communicate opinions which are rashly formed, on a cursory investigation of the paper, and therefore it would be an enormous benefit if we could get them earlier. I mention it here now, my lord, so emphatically because it is not the council alone, or the executive alone, that will enable us to do it next year. It is the members present who are going to read papers next year, who, if they would be kind enough to send them in very early, would confer on us a great benefit. Now, permit me to say on this point that I am myself one of the greatest sinners, and I make that confession with all due humility; but I join in agreeing that the evil is one that is very great, of gentlemen keeping back their wonderful ideas and their elegantly new language until the last moment. I beg to say that I think the paper of Professor Cotterill most valuable, and I do not talk on the subject, because I think nearly everything has been already said that is worth saying.

The **President**. I think I may now ask Mr. Cotterill to address himself to the observations that have been made, and I am very sorry myself, with many others, and Mr. Scott Russell in particular, that the discussion of this important paper should have been curtailed through the unfortunate fact that those who are good enough to send papers here for discussion do not always send them in good time. It has been suggested now in the course of this conversation that the council should address itself to this question. The council has done so, and in the course of the present session—only yesterday morning—we had a good deal of discussion on this subject, and considered whether or not there was any manner in which we could enforce more strict observances of our regulations, but it was considered to be a very difficult question. There is one law, no doubt, that we could make, and it would be a very effective one, which is, that any paper not sent in before a given day should not be read at a meeting. That, I have no doubt, would be a very conclusive sort of regulation, but the possible tendency of it might be this, that when the day of our meeting came we should have no papers at all. I am afraid that our friend Mr. Scott Russell has been known to be an offender in this case. In fact, he has said so himself, and while he is preaching to others, he himself has been a castaway. I think it would be well for him to bear that in mind. I am glad that the discussion has arisen, because everybody who reflects upon it must see that it really bears upon the utility of our proceedings here. It is a great object, one of our greatest objects, to have good discussions on the papers read, and you cannot expect to have good discussions on these difficult and scientific papers unless the members of the Institution have them to look at beforehand. Above all, I must thank our friend Mr. Scott Russell for the candor with which he has avowed his own faults, and I hope I may add for the frankness with which, like other naughty boys, he has said he will never do so again.

Mr. **J. Cotterill**. My lord, I can only plead guilty to the charge of sending my paper in too late, and, like a naughty boy who has seen the error of his ways, promise never to do so again.

ON TWIN SHIP PROPULSION.

BY G. C. MACKROW, *Member*.

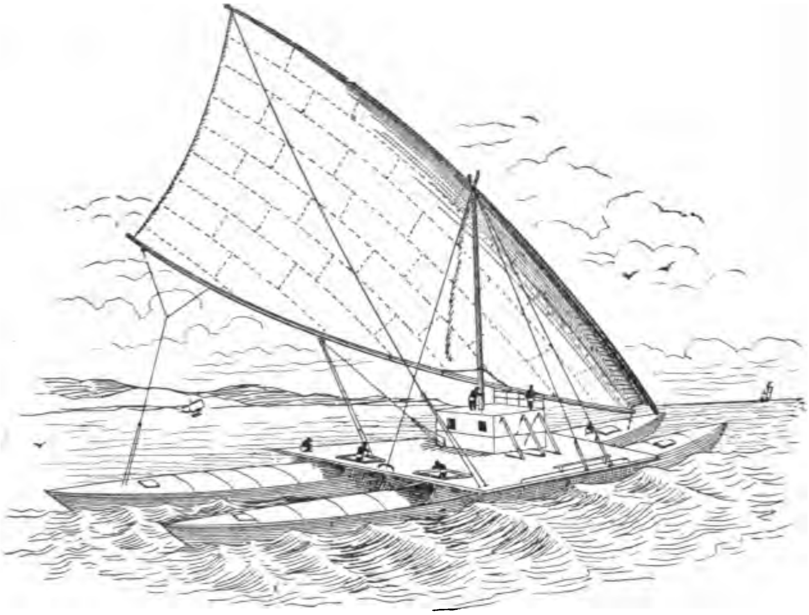
[Read at the twentieth session of the Institution of Naval Architects, April 4, 1879, the Right Hon. Lord HAMPTON, G. C. B., D. C. L., president, in the chair.]

In the year 1873, when the Channel Steamship Company applied to the Thames Iron-works and Ship-building Company, to construct a steamer on Captain Dicey's twin-ship principle, we endeavored to obtain some particulars of previous vessels built upon this plan, but nothing could be obtained but a most meager account of the *Gemini*, a wooden vessel which had been built some thirty years ago, and had plied for a short time upon the Thames. Others had been built in Scotland, but no reliable data could be obtained as to power, speed, &c., consequently the results to be realized in the new ship were always felt by most concerned in her construction to be of a doubtful nature.

The vessel, as you are no doubt aware, has been built, and I have thought the particulars of so novel a vessel, with a few remarks upon some phenomena connected with the movements of the particles of water about her hull, should have a place in the records of this Institution; and the more so when I bear in mind the fact that no system of construction has perhaps been the subject of so much criticism, both in the daily papers and scientific journals. It is needless to dwell upon the object Captain Dicey had in view in proposing this type of vessel, viz., for the purpose of reducing the horrors of the middle passage to a minimum, but let it suffice to say that, having been a captain in the Indian Navy, he had constant opportunities of witnessing the behavior of the flying proas or sailing craft of the Indian Seas, as well as the double canoes of the South Pacific; which craft, in order to their carrying a large amount of sail, have in the first case a pointed baulk of timber held at some distance from the vessel, by means of two spars, to act as a counterpoise when the wind tends to heel the vessel, as seen in the model on the table. In the second case, the natives build two complete canoes and connect them together, and these craft skim along the water at a great speed, and heel but little through the influence of the wind. (See illustration on next page.)

From this, Captain Dicey conceived the idea of two vessels secured by side, the one acting as a counterpoise to the other, each in its turn preventing its fellow from rolling. The question then arose as to the desirability or not of constructing two complete hulls, or two halves of one hull, and it was eventually decided to build two halves, as in the model on the table, these half hulls to be connected by a system of cross girders, the four engine and boiler-room bulkheads forming the main transverse girders which span the channel between the two hulls, and enter into the construction of the hulls from the keels to the upper decks, and the collision bulkheads at each end forming similar connections, the spaces between the bulkheads or transverse girders affording the passenger cabin accommodation. The *Castalia*, named after Lady Granville, was eventually thus built, and her plans and model I have the pleasure to submit to you. (See Plates XII, XIII, XIV.)

The *Calais Douvres*, a second vessel, has since been constructed with two complete hulls, without any advantage, it would appear, being derived from this system as regards speed, her co-efficient of performance being much lower than that of the *Castalia*, but having the disadvantage of throwing the extremities of the hulls some 18 feet further apart, thereby asking for additional material to make the end girders of equal strength, or with the same material to have less strength.



Dimensions of Castalia.

	Feet.	Inches.
Length, extreme	290	0
Breadth of each hull	17	0
Distance between the hulls	26	0
Draught of water as designed	6	6
Area of section of both hulls	209.5	0

Superstructure 168 feet long, 60 feet broad, 8 feet high, affording a magnificent promenade at a height of 14 feet above the water line.

The power of the engines as originally designed was 1,250 indicated horses, which it was hoped by the promoters would have given a speed of 14 knots, but of which I always felt doubtful, knowing that in such a type of vessel there must needs be a larger increase of surface friction and other retarding influences not found in ordinary vessels. I was not in any way responsible for the speed or power of the engines, as the contract for them had been previously signed by Messrs. J. & A. Blyth; the Thames Iron Works being contractors for the construction of the hull only. There are two pairs of diagonal direct-acting engines, one pair in each hull or half hull, with cylinders 46½ inches diameter, and having a stroke of 4 feet 6 inches.

The paddle wheels were common, 22 feet diameter over the floats, and the floats 20 in number, 10 feet long, and 2 feet 3 inches wide, with a dip of 3 feet 8 inches.

The boilers as originally designed, were 6 feet in diameter and 14 feet long, common, straight-away boilers, with one furnace in each, and eight boilers in all, namely four pairs. This arrangment was subsequently altered at the suggestion of the company's consulting engineer, to four boilers, 7 feet diameter and 6 feet long, with return tubes, and three furnaces in each. With this arrangement probably not more than 9 knots speed was obtained, as the boilers primed so badly that the engines could not be worked up to their full power, though many eminent men, the late Mr. John Penn among the number, did their best to obviate the priming, and eventually a contract was made by the Channel Steamship Company with Messrs. Maudslay, Son & Field for new boilers, and paddle-wheels with patent floats; these boilers, four in number, were 12 feet diameter, 10 feet 2 inches long, and the new wheels were 21 feet diameter over floats, which were 9 feet long and 3 feet 6 inches wide, with 4 feet 3 inches dip. This extra weight increased her draft to 7 feet, and the results of the measured mile trials gave a mean speed of 10.955 knots, with an indicated power of 1,516 horses, and a total area of midship section of 232 square feet, giving as her co-efficient of performance 200 on midship section and 100 on displacement, the lowest I had ever noted; and it is to seek for some explanation of the cause of this excessive waste of power that I invite the discussion of the members of this Institution.

Mr. Froude, in his valuable paper, read here in the session of 1877, on the effect produced on the wave-making resistance of ships by length of middle body, has thrown some light upon the subject, and as the *Castalia* had of necessity a long parallel middle body, in order to provide sufficient displacement, her bow-wave dies away before the run is reached, and so the benefit is lost of its power of propulsion. Up to the time of Mr. Froude's experiments being made public, it had been generally accepted as a theory, that once a channel had been formed to the extent of the largest area of section, the power needed to propel a vessel with a given length of parallel middle body, was comparatively small, amounting to merely that requisite to overcome the increased skin-friction. There can, however, be no question that the bow-waves when formed, falling in under the run of the vessel, very materially assist in her propulsion. There was, however, a very unlooked-for phenomenon noticeable the first day the *Castalia* steamed in the Victoria Docks, and to which I must draw your attention, as showing how mistaken are our thoughts oftentimes of what we have not actually proved. It was stated, I believe, by Mr. Reed and Mr. Merrifield, when the subject of twin ships was under discussion, that they believed the water would be carried away by the wheels from the channel between the two hulls faster than it would flow in, and consequently there would be a hollow formed just before the wheels; evidently assuming that the water would flow in at one end of the channel as it was driven out at the other, and this no doubt was a very natural assumption, but one that proved to be wholly fallacious, as I propose to show.

Upon the day of the *Castalia* first steaming in the Victora Docks, her bows were placed against a jetty on the north shore, and the wheels set in motion, when a very strong current of water was sent across the whole breadth of the docks, a distance of some 400 yards, setting the vessels rolling over on the south shore; and such was the strength of the current formed that the dock tug was unable to tow a vessel across it, and the *Castalia* engines had to be stopped in consequence. Now, any one witnessing the sight of this stream of white foam, flying like a millstream from the after part of the wheels across the docks, would very naturally expect to find a current of water flowing in at the other end and eddying round the stems from the outside of the vessel; to my astonishment, however, when I looked down between the hulls at the forepart, I found chips of wood floating perfectly motionless on the surface of the water, and I called Captain Dicey's attention to it. We at first thought the engines must have stopped; we ran aft, however, and found they were still working, but we had no means of seeing that day what was taking place nearer the wheels. Before the next day of steaming we got a boat beween the hulls, and Captain Dicey, myself, and the chief officer, with a few others got into her, not without some fears having been expressed on the part of some that the headfast of the boat was not strong enough to hold us against the current which it was believed we should find as we neared the wheels; and certainly as we neared them we appeared to be approaching a sort of miniature Niagara Falls.

The sequel proved, however, that there was not the slightest ground for fear, for while the engines were going full speed, and the water was driven at the rate, say, of 20 feet per second from the after part of the wheel, on the fore side up to the very point at which it was struck by the float, the water was all but motionless; a piece of batten was floated up to within an inch of the float and returned to us, was again thrust towards the wheel, was struck by the float, and instantly disappeared. Now, that the facts are stated, I have no doubt the members of this Institution will at once see that this is what we ought to have expected, for as the paddle-wheels scoop out the water a depth varying from 0 at the surface to 3 feet 8 inches at the extreme dip of the float, so the particles of water from below thrust themselves upwards at a velocity due to the pressures at the various depths, say from 0 pound at the surface to 228 pounds per square foot at the lowest depth. It may, however, occur to the minds of some that these features of the case are materially altered when the vessel is under way, as in that case the wheels are advancing into the body of water before them; but granting this to be the case the vessel does not advance at the same rate that the wheels remove the water, as their revolutions, say 20 per minute, give a speed of 23 feet per second, whereas the vessel would be advancing only at the rate of 18 feet per second. The deductions, I believe, then, we may draw from these facts are, that the particles of water that rush to supply those displaced by the revolutions of the wheels, come across under the hulls of the vessel from the outside in a partly athwartship direction, and thus create a considerable augmentation of resistance.

It may perhaps be thought by some that this is not a correct assumption. It appears to me, however, that seeing the water does not flow in from before, and that it cannot clearly flow in from behind, there remains but the sides as the only way of approach, the water sinking down from the surface to fill up the vacant space. Another phenomenon noticed upon a later occasion, namely, some sawdust thrown down into the wheels, was found coming up from under the bottom on the outside of the hulls, clearly showing that the water, being confined between the hulls, and compressed by the action of the discending floats, was carried down to the bottom of the hulls, where it was allowed to expand, and so was eventually carried across to the outer sides of the hulls, and then arose to the surface of the water, thus giving us a further clew to the enormously augmented resistance in this class of vessel. Were the wheel placed on the outside these two objectionable features would doubtless disappear.

This paper would not, however, be complete without a few words being said upon the steering qualities, as it was thought by many competent to judge of such matters, that she would not be found to answer her helm very satisfactorily. It was with some anxiety, therefore, that we at first cast off the Victoria Dock tug in that crowded part of the river opposite the dock entrance, and put her under her own helm. A few minutes were sufficient to show us that she was wonderfully under control. I noticed, however, later on, that she did not appear to pay any attention to her helm, even though in motion, until the stream of water from the wheels had reached the rudder, when the effect was most marked. That is to say, I have frequently noticed that when she had some way on her, the engines being stopped, that she paid no regard to her helm; but as soon as the engines were started, and the water rushed past the rudder, she obeyed her helm instantly; in fact, she has been so perfectly under control at all times that she is put alongside the piers at Dover and Calais with perfect ease. I show a plan of the rudders (see Figs. 4, 5, 6, 7, 8, 9, Plate XIV), to which I gave particular attention, so many misgivings upon the subject of her steering having been expressed. They work yoked together upon the Rapston slide principle, by means of a Brotherhood's engine and endless chain, one man being ample to work both rudders. Hand-steering gear is also provided in view of a possible failure of the engines.

I conclude by adding my own experience of her behavior, embodied in a letter I wrote to the Times in October, 1875, which I need not trespass on your time to read, but which I append with an outline specification giving her main scantlings, which will be found to be comparatively light, but have proved to have been amply sufficient. I should, however, not recommend any reduction in the scantlings for a similar vessel, unless built of steel and good material, and careful workmanship must of necessity be employed.

I append also a few particulars of the *Calais-Douvres*, culled from the public papers, and regret I cannot give more, though application was made to her builders through Captain Dicey. I have no doubt, however, that as far as they go they are correct.

Dimensions of Calais Douvres.

	Feet.	Inches.
Length, extreme	300	0
Breadth, extreme	62	0
Breadth of each hull	18	3
Distance between hulls	25	6
Draught of water	6	7½
Area of sections ... square feet	230	

Engines:

Diagonal, direct acting, with—

Cylinders 63 inches diameter, 6 feet stroke.

Wheels, 24 feet diameter, over floats.

Floats (feathering), 10 in number, 10 feet 6 inches by 4 feet 9 inches.

Boilers, 4 in number, 15 feet diameter, 19 feet long, fired from each end through 3 furnaces.

Indicated horse-power, on trial, 4,270.

Speed (mean), 14 knots.

Co-efficient on section, 148.

APPENDIX A.

[Extract of a letter to the Times, October 5, 1875.]

Until Tuesday last I had not been fortunate enough to be on board during any weather that would enable me to form a judgment of her behavior in a heavy sea. Leaving Dover, however, on the morning of that day, we encountered a strong gale from the southwest, which, being "abeam," tested her rolling qualities very fairly, if the term "rolling" can be applied to her movements. For, strictly speaking, she does not roll, but as each wave passes, the windward hull lifts to it and falls again, the wave passing across the space between the hulls to lift in turn the leeward hull, but not to set up a roll, for if it were possible to stop the next wave from passing she would remain motionless, whereas in any single-hulled vessel the oscillation set up would continue for some length of time, and she would gradually come to a state of rest, and it is the above peculiarity that gives its chief value to this form of vessel. These movements of necessity cause an alteration in the horizontal deck platform, but not more than 3½° on the above occasion, and it was the opinion of her able commander, Captain Pittock, that no weather she was likely to encounter on this station would give more than 5° of inclination, the present steamers rolling 14° to 15°. With regard to pitching and scending, I may add that while at Calais on the above-mentioned day the wind veered round to the northwest, and so on the return passage we had a strong sea ahead, into which the mail steamer was pitching, taking in the water through her hawser pipes and over her bows, sending it forth in white sheets over her waterways, while on the *Castalia's* low deck forward might be seen at work an engineer repairing a damaged float nearly the whole of her passage, her oscillation in a fore and aft line not exceeding 2¼°.

From the above remarks it is evident that so far as form is concerned, Captain Dicey has hit upon the right type of vessel for reducing the horrors of the middle passage to a minimum. It may, however, be fresh in the memory of your readers that various objections were raised some three years ago, when first this proposition was brought before the public; they were summed up in a letter to you, if I mistake not, by Mr. E. J. Reed, M. P., and reduced to five in number, which were as follows: First, plan objectionable, as involving too great draught of water; second, difficulty in connecting two hulls together; third and fourth, two vessels yoked together unfavorable for high speed; and fifth, the unusual breadth unadapted to the harbor of Calais. With regard to the first objection I have only to add that the *Castalia* complete, with all stores, coal, &c., on board, is drawing 7 feet of water, about the same as the mail boat on that station. With regard to the second, I need scarcely say that this part of the design received our first and most careful consideration, and was further submitted to that thoroughly practical body, Messrs. Lloyd's, receiving their fullest investigation and special survey of the vessel while building by their most eminent officers, and from all that we can discover from the time of launching (a severe test in itself) to the present time, after a most careful inspection during and after the gales of Monday and Tuesday last, not the slightest trace of want of strength is discernible, not a single rivet or bolt of deck, or waterways can be found, showing signs of straining; and this, coupled with the total absence of all vibration so common to steamers, may fully justify the conclusion that the second objection has been met. With regard to the third and fourth, however, we are obliged to yield, her coefficient of performance being a very low one, as the same power applied to an ordinary vessel would have propelled her at a speed of 14 knots. The placing of the propellers in a projected vessel is consequently giving us some difficulty, as the limited draught precludes the adoption of screws, and the extra breadth involved in placing the wheels outside is objectionable for several reasons, but not so much on the ground of any difficulty in entering Calias Harbor, the last of the series of objections raised by Mr. Reed, as the remarkable ease with which she answers her helm, coupled with the great lateral resistance she possesses, proves of the greatest advantage to her upon such occasions. It had, however, been felt by many naval officers, Lord Clarence Paget among the number, that she would be severely tested when strong easterly or westerly gales prevailed; her great height out of the water presenting so large an area to their influence, it was feared, after having partly entered the harbor, she might be driven to leeward, and thus thrown across the entrance. Of this, however, from the above qualities being possessed by her, it is now confidently felt there is not the slightest danger.

APPENDIX B.

OUTLINE SPECIFICATION.

The hull to be constructed on the longitudinal and transverse system, the plates to be capable of bearing a tensile strain of 21 tons lengthways, and 18 tons crossways. Each hull to be divided into water-tight compartments by means of eight bulkheads in each, the four central ones to run up to the height of saloon deck, and to be carried across the whole breadth of the superstructure and supported by partial bulkheads, thus forming a system of transverse girders.

Stems.—Of wrought iron 6 x 2½ inches.

Frames.—Of angle iron 4½ x 3 x $\frac{6}{16}$ = 2 feet apart.

Reverse frames.—Of angle iron 2½ x 2½ x $\frac{5}{16}$ inches.

Longitudinal frames.—Of plate iron 24 inches deep and $\frac{5}{16}$ inch thick. Angle irons on upper and lower edges, 2½ x 2½ x $\frac{5}{16}$ inches.

Floors.—Of plate $\frac{5}{16}$ inch thick and 26 inches deep.

Beams.—Of angle iron 6 x 4 x ⅜ inches on alternate frames.

Stringers.—Of plate iron 36 inches wide, ⅜ inch thick. Angle iron 3 x 3 x ⅜ inches. Lower deck, do. of angle iron 3 x 2½ x $\frac{5}{16}$ inches and plate ⅜ inch thick.

Deck ties.—On main deck 30 inches wide and $\frac{5}{16}$ inch thick.

Iron deck.—On top of superstructure $\frac{5}{16}$ inch thick, worked, jump-jointed, with edge and butt strips.

Bulkheads.—Placed where shown on plans, of plate $\frac{5}{16}$ inch thick, supported by angle irons 2½ x 2½ x $\frac{5}{16}$ inches placed 2 feet 6 inches apart.

Hull plating.—The strakes under first longitudinal $\frac{7}{16}$ inch thick, the other strakes up to main deck $\frac{6}{16}$ inch, as also the upper strake of superstructure; the rest $\frac{5}{16}$ inch.

Girder for plumber blocks.—To be formed of plate and angle irons as per sketch and to be sufficiently strong to carry the two outer ends of paddle shaft.

Wood decks.—Of pine 6 x 3½ inches.

Ceiling of hold.—Of elm 2½ inches thick. Room and space battens of pitch pine 1½ inches thick above this.

Boats.—Six in number, viz, four 25 feet cutter life boats, one 14-foot dingy, and one 20-foot cutter.

Anchors and chains.—Four bower anchors, Martin's patent, 15½ cwt. each, and 300 fathoms of 1$\frac{5}{16}$ inches chain cable.

Life rafts.—Two in number of plate iron, formed of a pair of rectangular tubes lashed a few feet apart, and two others on Captain Dicey's plan of wood and cork.

Sundries.—Awnings, &c.

DISCUSSION.

Mr. W. **John.** My lord, I do not think that a paper of this importance should pass without some one offering a remark upon it, and I rise the more readily because Mr. Mackrow has alluded to the society to which I have the honor to belong as having approved the construction of this vessel. Mr. Mackrow will remember when the drawings first came to us we discussed the matter with him, and there is one essential element in the construction of this ship which it would be well to point out to the meeting. I think it was a point we rather strongly pressed upon Mr. Mackrow's notice. It is as to the connection of the two sides of this model. I may say the weight of the hull of this vessel is kept down very low indeed, and it was a question of very great interest to combine those two hulls without adding considerably to the weight. I think I can explain the principle on which it was eventually done in this way: Owing to the wheel coming in the center of the vessel you see the two vessels are separate, and you cannot form a connection there, but by means of these two engine-room bulkheads put right across the vessel, before and abaft, you have a means for forming a connection—you have, in fact, two sides of what can easily be made tubular girders as it were, and by plating over between the two bulkheads *here* and the two bulkheads *there*, in an athwartship direction, and with the roof of the tunnel forming the lower part of the girder, you connect the two hulls by two large tubular bridges, two perfect tubes, which assist and hold them both obliquely and transversely, and for that reason you will find in the design of this vessel there is a partial iron deck up *here* (pointing to the model), but it is a partial iron deck put for purposes exactly the opposite of those for which the iron deck is usually put. It is not for longitudinal strength essentially, but for transverse strength, and there are, as it were, strips of iron deck going across the ship. It is by means of that system of having two absolute trunks from ship to ship, bridging over the space between, that you get the absolute freedom from movement which there is in that ship. I only rise to make that clear, because it is a point of very considerable technical interest in connection with a vessel of this light construction having the two hulls connected together, and yet, after working as it has done for some time, there is no sign of movement. The same principle, I may mention, has been carried out in pretty much the same way in the *Calais-Douvres*, which, with still more powerful engines, going at a higher speed, still keeps that rigidity, and shows there is combined with lightness, and a very peculiar shaped ship, and very trying circumstances, a sufficient amount of rigidity.

Mr. HENRY LIGGINS. My lord, I listened with very great satisfaction to the expression of the hope which is generally felt, I believe, in this Institution, that the mercantile service ships should receive more attention than they do, and the scientific features in connection with those ships should be considered as well as those of Her Majesty's Royal Navy. If that is a necessity, which I believe we are all agreed upon, it is very desirable indeed that ship-owners should consider it in a pecuniary point of view; and it is also important that the difficulties which attend the channel service for the transit of passengers should also receive the consideration of this scientific body, because the public at large do hope that out of the science of this Institution some suggestion may be made of a practical nature to get over the difficulties of the channel passage between England and the continent of Europe. The subject under discussion has been brought before this Institution during the last ten years by the most eminent of its members, notably Mr.

Scott Russell, who claims the right to speak with very great authority, having constructed the largest ship the world has yet produced. Mr. Reed has also addressed you upon it. The late Mr. Grantham also brought before you a very able paper, and I quite remember some years ago making a humble effort to contribute to the literature on the subject by reading a paper. I think I can state, without fear of contradiction, that I am the only person belonging to this Institution that ever made a passage in the *Gemini*, the vessel having proved a complete failure. And there I will correct Mr. Mackrow, if he will allow me, and ask him to strike out the word "ply" from his paper in connection with the *Gemini*, because she never arrived at that stage; she never went but once down the Thames, and as far as I was concerned she only brought me half-way back again. She belonged to a friend of mine, who built her believing, from what he had heard, that a vessel with a widely spread out deck would be a favorite with the public for pleasure traffic; he believed that with paddles placed between two hulls he could get high speed and large deck area. He spent £14,000 on this little vessel in the hope that he would be able to eclipse the large fast steamboats of that day employed between London and Gravesend. We struggled down the river Thames and got to Gravesend; we remained there an hour, and we endeavored to struggle back again, but only got to Erith, when, by universal consent, the visitors were glad to leave and get back by the next up Gravesend boat. She was a complete failure; she never made but that one passage, and never plied for hire. Now, we come to the next ship of similar type, the *Castalia*, which is before you, and I do not think Mr. Mackrow cares to claim much credit for the design of the ship. He was asked by Captain Dicey to build a ship with two hulls, and he thought out carefully how to carry out his instructions with enduring strength of structural form, and used much energy coupled with his scientific knowledge as a naval architect to produce the best result practicable under the many difficulties of this novel form of vessel. Lloyd's were consulted, because, no doubt, a great amount of dread was felt with regard to that form of construction, as to whether or not, in a heavy sea in the channel, the hulls might not separate from each other; therefore Lloyd's were consulted, with the view of getting an opinion on the strength of the girders that might be deemed necessary to keep the two structures rigidly together. The ship went to sea, I think Mr. Mackrow said, in 1875, and she did ply that summer between Dover and Calais, with a considerable amount of success as regards the satisfaction which, for a time, she gave to the public, but as soon as bad weather set in she was withdrawn, and from that time to this I believe the vessel has been lying in the Thames just below Woolwich. There were reasons why it was considered by her owners desirable to construct a ship of the same type, but of a better form. They asked for tenders, and a vessel was built called the *Calais-Douvres*, but that vessel is in no respect superior to this, and I think the two hulls of which she is made, there being two half hulls, is not an advantage which any one is able to appreciate. I have taken a good deal of pains to watch the working of the *Calais-Douvres.* It was a long time before I got a chance of seeing the *Calais-Douvres* in anything but smooth water, and a smooth-water passage across the channel from Dover to Calais is not one that any other boat cannot do with satisfaction to the passengers; but in September last I was able to catch the *Calais-Douvres* on two days when it blew rather hard.

I was on the Admiralty pier on one occasion when there was a good deal of cross sea, the wind was about WSW., and the tide was ebbing, which many gentlemen here know would produce very rough water indeed in the neighborhood of Dover. After waiting some time the Ostend boat arrived, one hour and forty minutes before the *Calais-Douvres* made her appearance, and the trains were kept waiting one hour and forty minutes for the arrival of the *Calais-Douvres.* She came through the mist which enveloped her and showed herself, and I watched her with the keenest interest. She rolled, or she moved—Mr. Mackrow does not like the word rolled in connection with her, but it appeared to me that she did roll—not an honest roll, but I may call it a wobble, which if not the ordinary roll of a channel steamer, was quite as objectionable to her passengers. She approached nearer and nearer, and with the aid of a powerful glass I watched her keenly, and she was coming straight end on to where I was standing on the Admiralty pier, and the part between the two hulls seemed to me to be completely choked with water, which sufficiently accounted to my mind for the very slow progress she was making in comparatively smooth water for so large a ship, she being 300 feet long. You would not have expected so much motion in an ordinary channel sea with a ship of that length; but she seemed to me to be completely choked between the two hulls, and that was enough to prevent her making rapid progress. She took green seas all over that lower deck. In fact, I very much doubt if it would have been safe to have stood on that day on that lower deck. Not having been on board, I am only speaking of what appeared to me to be the case from the Admiralty pier. She came in, and I must say that she steered very well indeed. I was told that there were 460 passengers on board, and I should think from the observation which I had the means of making that that was about the number, but I do not hesitate most solemnly to declare that at least 400 of those were sea-sick, for I do not think in my forty years' experience in the English Channel, or in my forty years' experience of crossing the Atlantic, I ever saw so many people sea-sick in one day as there were there. My son was with me, and we both counted the people as well as we could, and we afterwards noticed that a man and a boy were employed on the deck of that ship for half an hour and upwards in washing up the basins which had been used. That is very clear proof that those travelers that day had not derived any benefit from a slow large ship. There was not a particle of her deck that was not wet with the spray blowing over, and she is not protected as a comfortable vessel ought to be from the wind or the spray, because there is nothing but wire-work all round, just to prevent you tumbling overboard; there is no protection for ladies, whose dresses are apt to be blown up on the deck of a ship if they are not screened by high bulwarks. The ordinary channel steamers have very high bulwarks, and they are a great protection against the weather, and they put up weather-cloths fore and aft which keep off the wind and the spray, but there was nothing of the kind in the *Calais-Douvres*. I went below to examine every part, to see her under circumstances of a roughish passage, although I had seen her before. The cabin windows had all been closed, and the heat and offensive smell in this part of the vessel made it utterly im practicable for any one to remain there. It was impossible from the heat of the boilers that anybody could have remained for a quarter of an hour in those cabins without being almost parboiled. I assure you that I do not exaggerate in any word that I use; the heat was most excessive, and at this moment, if you go on board the *Calais-Douvres* you will find that the partitions and the bulkheads, or many of them, are cracked, and all of them are blistered from the excessive heat from the boilers. They are close to the boilers in the lower part of the vessel. That is an objection on rainy days, when there is no suitable place to go below and lie down. That is the *Calais-Douvres* as she was last autumn. Mr. Mackrow says that she steers well, and I say that she did steer well that day. She steered very carefully, and came to an inch alongside the wharf, but I can tell you this, that they dare not take her on rough days into the harbor of Dover; and on that occasion she had to go to the Downs to lay up for the night, and came back the first thing in the morning to take up her position. She does not go into Dover Harbor to rest as the ordinary boats do, and therefore I suspect they are afraid of her not steering with the accuracy which is necessary for the very narrow harbor of Dover. It is known to be an awkward place, and the tide is very strong at the harbor's mouth occasionally in certain states of the tide. It is perfectly impossible that it can answer to take a ship to the Downs every night, to lay up there until the next morning, and that then she should have to come back that distance of seven or eight miles to commence her morning's work. It is harassing, to say the least of it, to the crew—who, by the by, were glad enough to get out of her. One of the men said, "Well, we are here to-day and gone to-morrow;" they cannot get the crew to stand by her on account of the many inconveniences which they suffer, and the labor which they have to go through. I do not know that there are any other points that I need touch upon. I may here observe that the system of the two hulls is a very ancient one. The services of a catamaran like that vessel under sail might be very valuable for the carrying of letters and dispatches from ships through the surf at Madras, or through the surf on the beach of the South Sea Islands, where the water is very shallow, but it would be unsuitable, from the ballast it requires to render her stable enough to carry a press of sail for any other purpose. It by no means follows that such a frail boat as that, which is most useful in its way, should be applicable to the carriage of passengers and merchandise in the English Channel. The catamaran is really for beaching in shallow waters, and only carrying a man or two with dispatches to and from the ship. She is not intended to carry a cargo, nor is she capable of it. But it is a very ingenious unscientific method, because a savage invention of giving stability, but stability need not be placed in such form for the Channel service.

Mr. E. J. **Harland**. The particulars with which Mr. Mackrow has furnished us with reference to the results of trial trips, more particularly the *Castalia*, I think are very interesting, more especially not with regard perhaps to the probability of naval architects being able to reduce the principle of a twin boat to practical use, but rather to indicate to us in what direction we, in such applications or attempts, may fairly look in the future for a loss of power. It appears to me that one of the principal reasons why a rather serious loss of power has arisen in these twin boats may be traced to skin friction. It would almost point to this, that if a twin boat could be closed together a little at the bow, and made a little wider at the other end, the probability is that that would be an advantage so far as going ahead is concerned, but as both ends are made alike in order that the vessel may be saved having to turn, it means that the ship would have to be reversed again in returning. That is with reference to the matter of skin friction. As to skin friction, a little circumstance occurred in my experience which may be interesting. Some years ago a gentleman—I think Mr. Fletcher, of Liverpool, an extremely able and scientific man—brought out a speed indicator, and this instrument was on the principle of running what he called a sword through the bottom of the ship, two, three, or four feet in length, and at the bottom of this sword there was a hole bored in the direction of the vessel, so that when the vessel went ahead the water was driven against the surface of this hole, and an indication was taken by a pressure-gauge in the vessel, and he could thus, by an ingenious mode of calculation which he proposed to adopt, indicate the different speeds of the vessel. You could read in the cabin, or in any other part of the ship, the speed at which the vessel was going; it was a continuous speed indicator. But it met with a very unexpected circumstance. This sword, which was easily passed through a stuffing box at the bottom of the ship, was first made 2 feet in length, and when it was found that the speed indicated was only about half what the speed really was, the length of the sword was increased to over 3 feet; but it was still found, even at 3 feet or 3 feet 6 inches distance from the skin of the ship, 100 feet from the bow of a vessel something like 450 feet in length, that the indication was very far below what the ship was actually traveling, showing that there was a surface of water following that ship at that part of the ship of fully 3 feet in thickness; it did not mean that to the extent of 3 feet that water was flowing at the rate of the ship, but it meant immediately next to the ship perhaps the water was traveling almost identically at her own speed, and every foot you left the ship so much the water lessened in speed with the ship, but even when you had gone to 3½ feet from the ship you had not arrived at a point where the water could be said to be undisturbed. That indicates to me very clearly when we are dealing with naval structures, particularly where, as in this case, we have an extraordinary amount of skin surface wetted, that we then have to prepare ourselves to meet with a very serious objection, an objection which perhaps may weigh, and in this case appears to outweigh, any of the other advantages for which we are anxiously seeking.

With reference to the strengthening, to which Mr. John has referred, that struck me as most ingenious on the part of Lloyd's. Now, with regard to the paddles, that is another illustration of skin resistance Some little time ago we had occasion, in the construction of a paddle steamer, to go into an idea proposed by Mr. David Rowan, of Glasgow, a most ingenious and excellent professional gentlemen. In most cases it has been customary to have the float of the paddle about 3 or 4 inches clear of the ship's side. We found there was a positive advantage gained by cutting off the float next the ship's side, so as to be well clear of the shell of the ship, so that although apparently we lost a considerable amount of float for the applying of the power, still we really, with what remained, gained a better result, and we got a stroke or two more out of the engines for propelling purposes than when the float had a greater surface and was nearer the ship. That, in my mind, also points in the same direction. It shows clearly to me that the skin surface friction was such that at least for a foot or two outside the beam of the ship the water was following the vessel, so that the paddle really was doing an extra amount of duty, and in fact was pulling against itself next the hull of the ship. Now, with reference to the cross-channel trade and the cross-channel steamers, and the hitherto most unsatisfactory solution that has been offered, it is very unfair to naval architects, because they are blown up right and left for not putting something better on between England and France, when there is something so extremely satisfactory between Holyhead and Kingstown. To my mind it appears to be simply this. It is a matter of size of ship. You cannot have comfort at sea unless you have size, and in that size you must not forget the length. Now, that means a large harbor, and sufficient water in that harbor. Unfortunately, whatever has been done at Dover, is only one side of the hedge. At the other side, in the French ports, not only of Calais, but Boulogne, they are fearfully behind the day, and unless we first have satisfactory ports, the naval architect need not be asked or attempt to construct a satisfactory ship. Size is the only solution, in my opinion, of the difficulty. I do not think it will be in the direction of twin boats. There are complications about their first cost, and other difficulties, which I fancy would result in our adhering rather to the old system of single hull. I am quite sure that there will be no satisfactory solution of the problem until the authorities on the French coast as well as on the English have settled it by giving us sufficiently large and commodious harbors, and when that is done the naval architect will very soon try his hand, and, I have no doubt, will succeed.

Mr. **William Parker.** My lord, as I have taken a little interest in the propulsion of these two twin vessels, I have listened with pleasure to the remarks which have been made, and, with your permission, I will make a few observations about them both. There is one thing that has been settled beyond a doubt by these two costly experiments, and that is, that naval architects or enthusiastic patentees will never again imagine that this form of vessel will be the ship of the future. You will appreciate this more readily when I mention the enormous power that is necessary to propel vessels of this form, and the amount of coal that has to be burnt to obtain that power. The *Calais-Douvres* will burn about 220 tons to propel her 14 knots an hour, and you will agree that it would be suicidal to attempt to burn anything like that in a merchant ship. Now, there is one thing in connection with these two vessels, especially the *Castalia*, that strikes me, and that is, that before attempting to build the vessel some proper means were not taken to arrive at the power necessary to propel her 14 knots. It was very well known by the promoters of the company, I presume, that unless this ship did steam 14 knots she would be utterly useless. Mr. Mackrow says he did not expect that 1,250 horse-power would give her that speed, but he said the promoters expected to get it. Now, I think a very great blunder has been made by some one in this respect, and I will ask Mr. Mackrow, in passing, who was responsible for the power, and who was responsible for that power giving the required speed? The other vessel, the *Calais-Douvres*, was built by Mr. Leslie, of Hebburn-on-the-Tyne, for the same company, and intended to be run in the same trade. But what did he do when he commenced that ship? Having ascertained that it would be impracticable to run a vessel into those harbors over 300 feet long (10 feet longer than the *Castalia*), and that he could only be allowed 2 feet extra beam and the same draught of water, he at once took measures to ascertain what power was necessary to propel such a vessel at the required speed. Two Woolwich steamers were chartered, they were taken to sea, lashed together, and a set of progressive speed trials were made with these two ships. He then disconnected them, and ran them singly, taking particulars of their performances. Having obtained a certain amount of data from this experiment, he took the *Castalia* out, and conducted a series of speed trials with her. From those two experiments Mr. Leslie had collected sufficient data to enable him to say, neither 1,250 horse-power, nor 2,000 horse-power is sufficient to propel this form of ship; but I must construct engines so that they will be capable of exerting at least 3,600 horse-power. Mr. Leslie returned to the Tyne, he designed his ship, and he gave the engineers the power he would require to propel her 14 knots. The vessel was built, and the engines were constructed to develop that power, and when she was launched, with all her coal, and fittings, and everything on board, she drew about 6 feet 10 inches, being 2 inches less than the required draught; but the difficult problem Mr. Leslie had to solve was, having only 10 feet more length, and 2 feet more beam, how to get 250 tons more displacement in the same draught, which the extra weight of machinery required. He overcame that by an ingenious arrangement of the rudders. Instead of the vessel being very fine at both ends, he made her very full, and arranged the rudders so that they should be part and parcel of the vessel, being about 5 feet in width at the after ends, each rudder being about 10 feet long. His vessel had about 20 feet more midship body than the *Castalia*. It was also to get this extra displacement that the elliptical sides were adopted.

Mr. J. Scott Russell, F. R. S. Were both ends alike?

Mr. Parker. Both ends were alike; that was, 20 feet in length. As I said before, the vessel was built and launched, and she only drew that draught of water. She was tried at sea, and she indicated 4,100 horse-power, with a speed of a little over 14 knots; her bottom was very dirty, having lain in the water for sixteen months. After her bottom was cleaned she had a speed of about 14¾ knots on an indicated horse-power of about 4,300. Now, I have no hesitation in saying, having attended all the trials of both the *Castalia* and *Calais-Douvres*, that the *Castalia* is driven a great deal easier than the *Calais-Douvres*. But that was not the problem; the question was, can sufficient power be put into a vessel of these dimensions and this form to propel her 14 knots, and how can the extra displacement be obtained? This displacement could not be obtained without elliptical sides. Mr. Leslie had to produce a ship which burnt a great deal of coal, but still she did her work. The *Calais-Douvres* ran last season between Calais and Dover, and carried altogether about 51,000 people, which was about 700 a day. She earned nearly £22,000, and she cleared 15 per cent. of her original cost, after all the expenses in connection with working her, including interest and depreciation, were paid. If that is not a success I do not know what is, so far as doing her work is concerned. I hear from the London, Chatham and Dover Company that they are inundated with letters as to when the vessel will be ready to run again. I think Mr. Leslie, who undertook this piece of work, deserves great praise and credit; because when he commenced this boat he contracted with the Channel Steamship Company, and two months after he commenced it the Channel Steamship Company failed. Mr. Leslie went on with this ship at his own cost, and with this prospect in the future, that if she did not attain the speed he would have to break her up;

and if she did attain the speed he would have to look out and get somebody to buy her. I think he deserves great credit for the bold manner in which he carried it out, and I think the London Chatham, and Dover Railway Company deserve very great credit for at once taking it up, buying the boat, and giving the public a better means of transit between Dover and Calais.

Mr. J. R. **Ravenhill.** My lord, I should not have troubled the meeting if it had not been for a remark or two which have dropped from Mr. Parker. The first question that I should like to put to him, so that I may feel quite sure that I am correct, is this: I understood him to say that the *Calais-Douvres* burnt 220 tons of coal a day.

Mr. **Parker.** Yes.

Mr. **Ravenhill.** I believe she only made one passage across the Channel and one back each day?

Mr. **Parker.** That was all.

Mr. **Ravenhill.** Then, taking her full time to be an hour and a half, on each passage 60 tons an hour would make 180 tons a day—40 tons for getting up steam and lying-to. That is an amount of coal which it appears incredible could have been supplied to the fires in the time named.

Mr. **Parker.** Twenty-four hours is considered to be a day at sea, Mr. Ravenhill.

Mr. **Ravenhill.** I beg your pardon. I was thinking of the channel service, and when you said 220 tons a day I thought you meant 220 tons during the time in each day she was under steam or had her fires lighted. The thing puzzled me and I am glad I have elicited that explanation.

Mr. J. **Scott Russell.** It is about 10 tons an hour.

Mr. **Ravenhill.** I think there is one thing about these twin steamers that have been placed in the channel, and it is this: There can be no doubt that the horrors of that passage had been experienced by the traveling public over a long course of years, but I do not think you will see that class of ship multiplied. I am of opinion that if larger vessels were placed on that station built of steel, such as the *Maid of Kent*, and some of those built many years ago, when first the London, Chatham and Dover Railway opened, or if those vessels were enlarged, you would obtain nearly all, if not quite all, the advantages to be derived from these twin steamers, and with very great saving, as far as fuel is concerned, at all events. Mr. Harland alluded to the great skin-friction. He did not put it quite before you in the point of view in which I should like to put it, which is this: In the case of the *Calais-Douvres* there is just twice the amount of skin-friction that there would be in the ordinary hull of a vessel of the same beam, and that in itself must be fatal to speed. I believe there are many naval architects who, if they were consulted, could give you a vessel built of steel at the present time, say, length 300 feet, and breadth over all the same as the

Calais-Douvres ; but they would, instead of spreading the hulls, bring them together, and take the dimensions all over the ordinary old form of paddle-boxes. There might be the difficulty of turning that was alluded to by a gentleman who spoke some little time back, at all times on entering Dover Harbor; but if the public drive the railway companies into giving them accommodation across the channel at the present time, when the accommodation in the harbors is of a very limited description, the companies must put up with the necessity of their vessels going into the Downs, or elsewhere, for the night, to anchor. That is a matter that only affects the railway companies themselves. I do not think it at all affects the question as to the relative capabilities and performances of the ships. I believe that before very long we shall see improved vessels on the Dover station, but they will not take the form of the *Calais-Douvres.*

Captain Dicey. My lord, I wish to make a few remarks with regard to what Mr. Liggins said about the channel service. I have been here several times on previous occasions when this matter has been discussed, and I have always found Mr. Liggins throw cold water on every scheme that has been introduced. I should fancy Mr. Liggins, with the experience that he has had across the channel, and to America, must be like one of those sailors on board the ordinary channel steamers whom I happened to speak to some time ago, and said, "Cannot you get a better class of vessels to run across here?" "Oh," he said, "the passengers get used to it; they would not care about any improved steamer at all." With regard to the steamer going to the Downs, I watched the *Calais-Douvres* very frequently, and last year, during the time she was running, three months or three and a half, she went to the Downs only twice. That was in consequence of there not being water enough in Dover Harbor to enable her to go into the harbor, but every other time she anchored under the pier or went into the harbor. Captain Pittock said over and over again, "I can assure you, Captain Dicey, I would as soon navigate this vessel as one of our ordinary channel-going steamers." As far as the difficulty of the ports is concerned the great difficulty is at Dover, not at Calais. You can always get in and under shelter at Calais, though you may have to wait an hour or two for the tide, but at Dover the harbor is dry at certain times, and therefore you are obliged to wait, and of course the steamer could not remain outside if it were blowing a gale of wind. With regard to the heavy passage Mr. Liggins has spoken of, I went down the day after the vessel crossed, and I asked the captain and others aboard what kind of passage they had. They said, "A very rough passage;" and in fact it was so rough that the channel steamer could not cross that day, and the Folkestone and Boulogne steamer could not cross that day; she was kept in Folkestone on account of the heavy weather, whilst this vessel crossed over with great comfort to her passengers—so much so, that they told me there had been very few sick. As a
proof of that I spoke to the steward.
I said, Steward, you have no—

indicator on board this vessel; we have them on the *Castalia* to show what the incline would be in rolling." And he assured me that they never rolled, and though many of them had made a comfortable dinner on board, the ship had never rolled a bottle off the table. The inclinometer, the apparatus for testing the roll on board the *Castalia*, never showed more than five degrees of rolling, when other vessels rolled to a very great extent. Now, with regard to what Mr. Liggins said as to not being able to see anything in the channel between the two hulls when the vessel came into Dover, and that it was entirely choked up with water, the fact is, that the paddles block up the passage right across. The distance across, I think, is 22 feet, and the paddles are 21 feet of the whole distance across, so that it is impossible that could be seen through, unless you could see through the whole thickness of the paddle-box.

Mr. LIGGINS. I did not say I could not see through, but what I said was that the channel was blocked between the two. The trunk between the two vessels was choked with water from the cut-water to the paddle-wheel. Captain Dicey and I cannot be talking of the same day, because there is a gentleman here who was a passenger on the day to which I have alluded, and he can confirm my statement.

Captain DICEY. The vessel only rolled 5°, when other vessels rolled much more—14° or 15°—that is a fact that cannot be denied. In the report of the London, Chatham and Dover Company which they sent out to all the shareholders printed in a small pamphlet, they say that the *Calais-Douvres* always made a faster passage than the *Maid of Kent*, their best vessel. The *Maid of Kent* was always looked on as one of the fastest vessels in the channel, but it so happens that the *Maid of Kent* on one occasion crossed over with the *Calais-Douvres*, and the *Calais-Douvres* made the passage in something like ten minutes less than the *Maid of Kent*. Those facts speak for themselves. With regard to one or two occasions when she made rather a long passage, that was entirely owing to the steering apparatus. They steered by steam, and the steam-gear frequently got out of order, and detained the vessel both on the trial trip and on two or three occasions afterwards. Besides, I can safely say she never went into the Downs more than twice, I think it was, and Captain Pittock says she was a vessel he would not hesitate for a moment about being out in in a gale.

A VISITOR. Mr. Chairman, allow me to say a few words in confirmation of what my old friend Captain Dicey has said. I myself was a passenger in the *Calais-Douvres* no less than seven times during the month of September last. I have had to do with Captain Dicey in different parts of the world—in India—and ever since I have known him very well. I joined him as much from friendship as speculation with regard to the *Castalia*, in which unfortunately I am a shareholder. I must say that a more comfortable vessel in a gale of wind no man who ~~though we hope large harbors will be made both on this side and on the French side, still certainly our grandchildren will not have an op-~~

has gone round the Horn or the Cape of Good Hope need be in, and had she capacity for carrying coals, I certainly would be glad to form one of the crew to go a pleasure trip round the world in the *Castalia* or the *Cailas-Douvres.* As to the unfitness of these vessels to cross in bad weather, I must distinctly contradict what I have heard said by a gentleman whose name I do not know, that there could be anything like disagreeableness in her. On one occasion when she crossed there were more than 500 people on board, and many of them were ill. In that case there were some relatives of my own, who before they left Calais were ill while the ship was lying in quiet water, and they were certainly very ill on their way across, as also were many others. I have knocked about the world in all portions of it, and in all sorts of seas, but I certainly felt no necessity to be ill. A great deal of spray came over the vessel, which anybody could see, but it does not require a sailor to tell you that if there is a large vessel which cannot possibly roll, the sea hits the side of that as it would do the Dover Admiralty Pier, and the spray must fly in, if the ship cannot give. Water takes its shortest route, as Mr. Scott Russell explained last night—water will rise to the surface by the shortest route, and will come down by the shortest route. The comparison of the two ships, of course, one has nothing to do with. I can quite verify what Mr. Liggins said about the uncomfortable feeling in the *Calais-Douvres* on going down below on such a day with everything open; the heat was perfectly unbearable. That I never experienced on board the *Castalia.* It may be there is something in the form of the *Calais-Douvres* which makes the heat in her so much greater in excess of what it is in the *Castalia,* but you cannot avoid it, and by the enormous sausage-boxes there are on the top of her boilers, which are very nearly as large as the boilers, a huge mass of steam is kept in the *Calias-Douvres* that does not exist in the *Castalia.* As to the comfort on deck, I think it does not require one who has crossed so often in her as myself or the word of Captain Dicey, or the word of Captain Pittock, whom I know well, who I am quite certain will speak of both the vessels with the highest praise, to show that if 400 people can go on board that vessel with plenty of air, they must have traveled in greater comfort than on the deck of a vessel that does not represent one-sixth of the superficial space. The deck must convince you all of it. As to the great comfort of the twin ship, that is thoroughly secured. As to the speed, that is quite another matter. Mr. Harland has explained from a ship-builder's point of view, I should say, that the whole arrangements were very unsatisfactory, but I think myself we need not despair of having a vessel presenting such a very large skin friction being propelled at as good a pace as a vessel with one-half, whether we get it by putting an enormous excess of horse-power, as Mr. Leslie has done in the *Calais-Douvres,* or by some other adaptation, whether paddles or screws, or something we know nothing about at present; but I feel convinced that attempts should be made to drive this form, because ~~proof of that I spoke to the steward. I said, " Steward, you have no~~
though we hope large harbors will be made both on this side and on the French side, still certainly our grandchildren will not have an ap-

portunity of crossing over in such vessels as go between Holyhead and Kingstown. The time it will take to build those harbors will see us all in another state of existence, and I think we must not despair of being able to propel these twin vessels at a speed far in excess of any boats, whether they run from Calais to Dover, or across the great Atlantic. As to the form of this vessel, I may remark that it is very old. I may mention that I am acquainted with a gentleman who was in this room this morning, a well-known engineer in the north of England, a relative of mine, who made experiments forty years ago on the hulls of twin ships. Some of the drawings of those ships I have seen. They commenced with the single hull cut in half with straight sides, and he then gradually altered the form, putting in slightly developed bows on the inner side, and he went on until he got both bows and both hulls equal. He found by actual experment that the straight line was that which with the same power gave the greatest speed. I think from what we have heard of the easy way in which the *Castalia* is driven with about 1,220 horse-power, nominal as compared with the enormous power of the *Calais-Douvres*, if the experiments were carried further by separating these large models, for every inch that they were separated from being one hull it would be found that the speed decreased with the same power.

Mr. G. C. Mackrow. I will say, gentlemen, that I had no thought of bringing forward this paper to advocate the system. It was merely to present to the Institution the results that I have noticed in connection with her. I think the system itself is before the public, and every one must judge for himself of its value. With regard to Mr. John's remarks, I may say I was very glad, of course, when so many conflicting opinions were abroad as to the construction of this vessel, to have Messrs. Lloyd's at my back. Every one who came to see the vessel at the works during her construction expressed their fears that we should come to grief sooner or later, and of course it was a responsible position for the servant of a large company to be placed in. And I was very glad to find that I had the full support of Messrs. Lloyd's in what we had undertaken. Then, with regard to Mr. Liggins' remarks, I believe Captain Dicey and others have fully met them. I might say, with regard to the remarks Mr. Harland has made as to the cutting away of the floats, that was the very first thing we did after the first time we went down the river. The very night we brought up at Gravesend, the first thing we did was to cut away some 6 or 8 inches from each end of the floats, as it was feared that the water was jamming between the ends of the floats and the ship's side. This cutting process went on until eventually we found her fitted with those patent floats of Mr. Aston, which consist of two 6-inch angle irons placed vertically side by side. I was not present at the trial, but it was reported that she had made the same speed as with the ordinary horizontal floats. With regard to Mr. Parker's remarks, or rather inquiry, as to who was responsible, I might say that Messrs. Blyth had undertaken originally the whole contract for the ship, and for the engines. Some experiments had been tried, I think by the Channel Steamship Company, on two vessels lashed together above bridge, but I myself have never attached much importance to those experiments, for this reason, that they were not tried under the same conditions as in the *Castalia*, inasmuch as you have wheels on the outside as well as the inside, and, I think, as I have endeavored to show in the paper, that it is owing to the wheels being placed on the inside that we meet with this enormous amount of resistance. I should be very glad indeed to see the engines reversed, and see those wheels simply placed on the outside of the hull. No one, I fear, will undertake any such experiment, but it certainly would be very satisfactory. I think we should find, then, no very great difference between the performance of this vessel and an ordinary vessel. Mr. Harland has shown us clearly at what depth below the hull of the vessel the particles of water are found in motion in connection with the hull. If you notice, in the paper I have drawn attention to, what I believe is the action taking place in an athwartship direction, and if that is the case, and we carry those particles of water down to 3 feet below the bottom of the hull, it actually amounts to an increase of the midship section of the vessel to another 3 feet of depth. Of course Mr. Leslie is deserving of all the credit that Mr. Parker gives to him, but certainly you must bear this in mind, that Mr. Leslie had the *Castalia* to experiment upon, which we had not. I think I have met the inquiry as to who was responsible. Of course we were hoping against hope. It was an experimental vessel. We undertook to build it entirely from a commercial point of view, and certainly when it was proposed to us we felt very timid, and we have sometimes to refuse to entertain propositions when they are so exceedingly wild; but seeing how desirable it was to have steamers suitable for the passage across the Channel, we felt disposed to run some risk; but it was not in the power of the directors of the Thames Iron Company to expend large sums of money upon costly experiments involving considerable outlay, especially as it was in a matter that they themselves were to derive no especial benefit from. Such experiments devolved upon the Channel Steamship Company. I can only thank you, gentlemen, for the kind way in which you have received the paper, and say this, that while I gave notice under pressure that I would contribute a paper some few months ago, I really left it until the last moment, but I should have been very glad indeed if a paper of more value had been forthcoming. When I heard towards the close of the time that nothing presented itself of more value, as the council judged, I then had barely time to prepare the plans and the paper; I have, however, presented it to you to-day in the best possible form that I could.

The PRESIDENT. We must all feel, I am sure, very much obliged to Mr. Mackrow for the very interesting paper which he has contributed, and for the interesting discussion which it has elicited from the gentlemen present.

ON THE STEAM TRIALS OF H. M. S. IRIS AND THE RESISTANCE OF SCREW PROPELLERS.

BY M. J. A. NORMAND, *Member*.

[Read at the twenty-first session of the Institution of Naval Architects, March 18, 1880; the Right Hon. Lord HAMPTON, G. C. B., D. C. L., president, in the chair.]

In a most elaborate and interesting paper read at the last session of the Institution, Mr. Wright has applied the theory of the late lamented Wm. Froude to the steam trials of H. M. S. *Iris*, and the result of his investigations is, that this theory cannot account for the great difference of performance of the different screws.

I have made a careful analysis of those experiments, and beg to show that the discrepancies observed are very probably owing to an insufficient estimate of the direct resistance of the blades.

Beaufoy's experiments (however antiquated this opinion may seem to be), are still capable of furnishing reliable data, and I shall take a few of the results of those experiments in order to show that in most screws the direct resistance is superior to friction.

In the report of Beaufoy's experiments, the total resistance of the bodies experimented upon is divided into friction, and plus and minus pressure or direct resistance. As the friction planks which were used by him to determine the co-efficient of friction were nearly of the same length as most of the bodies, the calculation is accurate, and it is not necessary to alter that co-efficient for difference of lengths (as shown by Froude's experiments). Accordingly, the direct resistance, or plus and minus pressure, as Beaufoy calls it, may be taken as accurate, and it remains to compare that direct resistance with the friction of the vertical sides only, as the top and bottom do not exist in screw blades.

The frictional co-efficient found by Beaufoy is very nearly that of tin-foil found by Mr. Froude for equal lengths of surface, but it will be better to take here Mr. Wright's co-efficients, which are much higher, especially on account of the small lengths of screw-blades surfaces.

These co-efficients are as follows:

For screws Nos. 1 and 2	0.00564
For screws No. 3	0.00345
For screws No. 4	0.00332

It will be sufficient to effect the calculation with the higher and lower co-efficients:

The table at end gives an exact plan of the bodies calculated upon, the direct resistance according to Beaufoy, the surface of the two vertical sides only, and the friction upon these two vertical sides found by Beaufoy, and that computed according to Mr. Wright's frictional co-efficients 0.00564 and 0.00332 respectively.

In order to show that the screw-blades of the *Iris* may be compared with Beaufoy's bodies, a section of those blades, taken at half length from the boss, is given in the same table.

If we compare body A P *a*, for instance, with the screws of the *Iris*, it seems very probable that the ratio of direct to frictional resistance of that body is less than that of screws Nos. 1 and 2, and about the same as that of screws Nos. 3 and 4; and we see from the table that this ratio is equal to unity with the larger frictional co-efficient, whereas it is much greater with smooth surfaces.

It may be objected that screw-blades travel obliquely, however small the angle may be, through the water, so that they are not exactly comparable with Beaufoy's bodies, and, also, that the sides of these bodies are not rounded and fair, as ordinary screw-blades are.

Altogether, and making due allowances, it remains extremely probable that, with smooth surfaces, such as those of screws Nos. 3 and 4, the direct resistance of screw propellers, instead of being only 10 per cent., as estimated by Mr. Wright, is greater than the friction. In what proportion, it is yet impossible to say, and experience alone can decide.

In accordance with this opinion is this fact, which I have every reason to believe true, that no great difference of performance has been observed in the *Iris*, whether the screw-blades were polished or rough from the sand.

It should also be observed that the direct resistance increases according to a higher power of the speed than the friction, so that, at 50 or 60 knots, a speed often attained by screw-blades, the ratio of the two resistances should be much greater than at 8 knots.

It is also extremely probable that the direct resistance increases as the thickness is reduced, or, in other words, that the direct resistance of a board 1 inch thick and 12 inches high is greater than $\frac{1}{12}$ the resistance of a body 1 foot square, with ends similarly shaped. At least, this is true for Beaufoy's short friction plank, with semi-circular ends, 3 inches thick, compared with a similarly shaped body 1 foot broad. At 8 knots, the direct resistance per square foot of the former is 1.60 that of the latter. A similar result is given by other bodies respectively 3 and 12 inches thick. Should this be true, and the matter could easily and with great advantage be decided by experiment, the edge resistance in screw-blades would be greater than assumed from Beaufoy's bodies 1 foot square.

Taking all this for granted. it may easily be seen that, instead of 1,000 horse-power being lost by the resistance of the *Iris*' screw, No. 1, at full speed, 2,000 horse-power is nearer the case. I have ascertained, also, that the direct resistance of screw No. 1 is about double that of screws Nos. 2 and 3, and more than double that of screw No. 4.

Besides, an important loss of power has been set aside by Mr. Wright, and this is the longitudinal component of both resistances. From a rough calculation, I believe this loss of power to be about 18 per cent. for screw No. 1, and 23 per cent. for screw No. 4, of the loss resulting from both frictional and direct resistance.

Two different kinds of experiments could be tried for solving the problem which forms the subject of this paper:

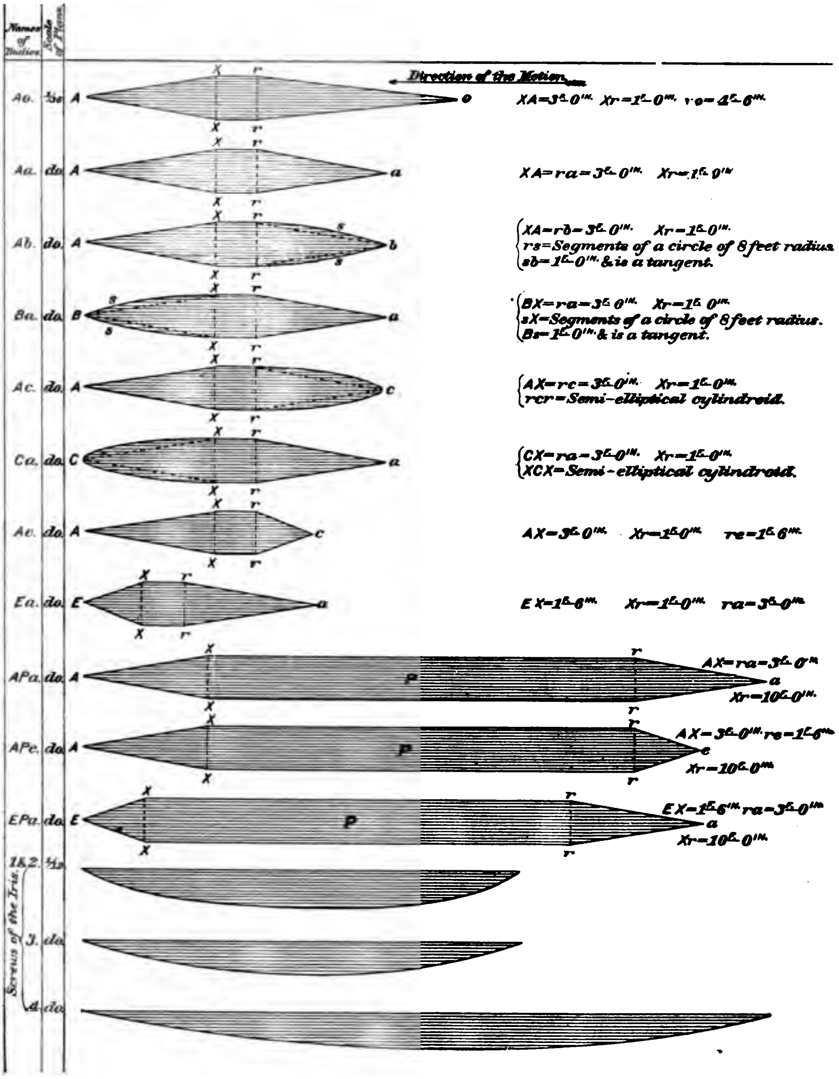
1st. To try screw propellers exactly alike, but with very different thicknesses of blades, and also the same propellers with very different polish, which could easily be done by mixing sand with paint.

2d. To measure simultaneously the resistance of planes shaped like screw-blades, and moving obliquely through water.

If the lateral resistance, that is to say, the resistance perpendicular to the motion of the planes, could be recorded at the same time, which seems relatively easy with the carriage used by Mr. Froude, these experiments would perhaps furnish sufficient data to establish a sound theory of the screw propeller.

PLAN OF BODIES

[The depth of each being 1 foot. Immersed midship section of each of Beaufoy's bodies = 1 square foot.]


Names of Bodies
Scale of Plans
Direction of the Motion
Ao. 1/36 A X r o
XA=3ft-0in. Xr=1ft-0in. r o=4ft-6in.
Aa. do. A X r a
XA=ra=3ft-0in. Xr=1ft-0in.
Ab. do. A X r s b
XA=rb=3ft-0in. Xr=1ft-0in.
rs=Segments of a circle of 8 feet radius
sb=1ft-0in. & is a tangent.
Ba. do. B s X r a
BX=ra=3ft-0in. Xr=1ft-0in.
sX=Segments of a circle of 8 feet radius.
Bs=1ft-0in. & is a tangent.
Ac. do. A X r c
AX=rc=3ft-0in. Xr=1ft-0in.
rcr=Semi-elliptical cylindroid.
Ca. do. C X r a
CX=ra=3ft-0in. Xr=1ft-0in.
XCX=Semi-elliptical cylindroid.
Ae. do. A X r e
AX=3ft-0in. Xr=1ft-0in. re=1ft-6in.
Ea. do. E X r a
EX=1ft-6in. Xr=1ft-0in. ra=3ft-0in.
APa. do. A X P r a
AX=ra=3ft-0in.
Xr=10ft-0in.
APe. do. A X P r e
AX=3ft-0in. re=1ft-6in.
Xr=10ft-0in.
EPa. do. E X P r a
EX=1ft-6in. ra=3ft-0in.
Xr=10ft-0in.
Screws of the Iris.
1&2. 1/12
3. do.
4. do.

Surface of the two vertical sides only.	Plus and minus pressure, or direct resistance at 8 nautical miles (the highest speed recorded in Beaufoy's experiments.) R	Frictional resistance against the two vertical sides only, at 8 nautical miles.			Ratio of direct to frictional resistance.		Ratio of length to breadth.
		According to Beaufoy's experiments.	Computed by Mr. Wright's co-efficients.		$\frac{R}{R'}$	$\frac{R}{R''}$	
			0.00564 R′	0.00332 R″			
Square feet.	*Pounds.*	*Pounds.*	*Pounds.*	*Pounds.*			
17.00	30.67	7.34	17.52	10.30	1.75	2.97	8.32
14.00	32.13	6.04	14.42	8.48	2.23	3.80	6.82
14.02	32.80	6.05	14.45	8.49	2.27	3.86	6.82
14.02	30.49	6.05	14.45	8.49	2.11	3.59	6.82
14.05	36.65	6.07	14.48	8.51	2.53	4.30	6.82
14.05	33.56	6.07	14.48	8.51	2.32	3.95	6.82
11.00	44.86	4.75	11.33	6.66	3.95	6.73	5.30
11.00	43.17	4.75	11.33	6.66	3.80	6.49	5.30
32.00	32.82	16.03	33.00	19.38	0.99	1.69	15.62
29.00	49.26	14.53	29.88	17.56	1.65	2.80	14.12
29.00	46.32	14.53	29.88	17.56	1.55	2.64	14.12
....	....	....	....	....	....	....	11.3
....	....	....	....	....	....	....	13.6
....	....	....	....	....	....	....	18.0

DISCUSSION.

Mr. J. **Wright** (vice-president). My lord and gentlemen, I should like to say a few words upon this paper. Mr. Normand has gone to considerable trouble in endeavoring to show that the discrepancies observed in the results of the trials of the screws in the *Iris* were probably owing to an insufficient estimate of the direct resistance of the blades. The subject is rather a speculative one, and the paper was only put in my hands a few minutes ago, and therefore I am not prepared to say much on the subject; but I would just remark that in estimating the co-efficients of friction they were taken, as near as we could judge of the different surfaces, from the experiments made by Mr. Froude on the surface friction of planes drawn through the water, and I followed Mr. Froude in his opinion on the subject, that direct resistance was but a small proportion of the frictional resistance. Mr. Normand admits that there is a good deal of doubt on the subject, and that it is impossible to say what the exact amount is, and that it can only be determined by experiment. A short time before the lamented death of Mr. Froude he had commenced a series of experiments on screw propellers, but I regret to say that these experiments have not been carried out as we had hoped they would have been carried out by this time; but it is a subject well deserving further consideration and experiment. I hope that in time, when the establishment in Chelston Cross is not so much pressed as it is at present, some experiments in the direction that Mr. Normand has indicated may be made, with the view of clearing up doubts that still exist with regard to the frictional and direct resistances of screw-propeller blades.

Mr. A. C. **Kirk** (member). My lord, I will just make one remark. It seems to me that the attempt to reduce the resistance of propeller blades from that of bodies moving on the surface of the water must be quite unreliable. A propeller blade is more like a fish torpedo swimming below the water, and we can attach very little weight to such experiments.

Mr. **William Henry White** (member of Council). I should like to say, in correction of Mr. Kirk, who, I am sure, would not misrepresent the point, that many of Beaufoy's experiments were made on submerged bodies.

Mr. **Kirk**. I am very glad Mr. White has mentioned that. It takes away all the point of my remark.

Admiral **Selwyn**. My lord, as an early advocate of the "twin" screws, to which I believe I gave the name in this hall, I wish to point out that there is a question very important in the discussion of the resistances to be encountered in the movements of those screws. Around every screw there is carried a skin of water, so to speak, which is diverging and radiating directly from the boss, and which is not thrown astern at all. It is part of the resultants of the action of the screws in the water. If, when the particles of the water set in very rapid motion indeed, as they leave the exterior of the blades of the screw in a radial direction, encounter a resisting body close to them like that of a ship, against which they can exercise friction, a notable loss of power will be the result. It is not necessary only to study the friction of the blades themselves, but also how the water passing off those blades can find free issue without increasing the resistance. For that reason, in the early experiments of the double screw propeller, as it was then called by Mr. Dudgeon, I urged the separation of the two shafts at the after end, because I thought that the loss by the parallelogram of forces would be less than that to be encountered by the closeness of the blade in its revolutions to the run of the ship. I think, too, that a good deal is to be said on that question of the *Iris* investigation, and that it should receive very close attention. If the screw is entirely removed from the ship and not subjected to those forces it will give one set of results; if, on the contrary, it is close to the surface of the ship, it is quite certain that it would give a totally different set of results, although it may be a screw of exactly the same diameter, pitch, and thickness of blade.

Mr. **Griffiths**. I have been making some experiments lately with regard to frictional resistances of the screw, and therefore, my lord, perhaps I may be excused if I say a word or two. As to the screw itself, I believe it is of very little consequence whether the blades are thick or thin. The first time I ascertained that was with a screw which I put in the *Great Britain*. When she came back from Australia I found on the propelling side of the screw that there was weed three inches long, consequently there could have been no friction on it. It does not matter, as I have already several times explained, what the form of the screw is. If you put a bit of plate iron, in the form of a blade, on a boss, setting them at an angle which will keep the power at the speed you want, you will get as good a result within a quarter of a knot as from the best screw ever yet made. It is on the position of the screw that its efficiency depends. I have fitted five or six ships with the screws right against the stern post, as near as they could be put, giving them, of course, some little room to move. In every case we have got from one-quarter of a knot to three-quarters of a knot more speed, though the screw was merely shifted about 20 inches, or from that to 2 feet. But, if by experiments which come out the same in the model as in the ships, the screw is moved equal to two-thirds of its diameter back, I get from 10 to 15 per cent. more speed with the same power and the same revolutions. Then if I, beyond that, move it a certain distance, I lose speed again. It is where the two streams join that the screw ought to be, so that the screw is fed with water. They got more speed in the *Iris* because they reduced the screw and kept it further off the skin of the ship. There is always a volume of water which follows the skin of the ship, and if the screw sucks that back it sucks back the ship.

Sir R. SPENCER ROBINSON, K. C. B., F. R. S., admiral (vice-president). My Lord Hampton and gentlemen, perhaps I might be allowed to say a few words. It seems to me from this discussion, which has been an extremely interesting one, coming after the rather extraordinary results, first arrived at from the trial of the *Iris*, that we are now exactly in a position to say that the reasoning and arguments upon the shape of the propellers are in a very unsatisfactory condition indeed. What has been alluded to three or four times by the other speakers, who have preceded me, is really the very thing we want. We want experiments made on screw propulsion, on the forms and shapes of screws, and their adaptation to the various ships. I sincerely hope that the admirality, who directs those things, will, as has been said already by some of the preceding speakers, cause such experiments to be made as will remove that doubt, just as experiments have been made which have removed the doubts as to forms of least resistance, and as to the mode in which broad ships, with equal displacement, are equal to narrow ships. I have no doubt that the experiments properly conducted will remove that reproach on the sciences of naval architecture and engineering which now rest upon them, because it is a reproach on the sciences of naval architecture and engineering that we should be so considerably in doubt as to the forms of our propellers, as to have led to those results we have seen—a propeller exchanged for another, the blades cut off, and finally, results perfectly satisfactory obtained, after trials have been abandoned, by a system of rule of thumb, rather than by any scientific application of the laws of nature.

Mr. White. In the absence of the writer of the paper I cannot pretend to speak for him, but I wish to say a few words with reference to the remarks of Mr. Griffiths and Admiral Selwyn, who are of opinion that the distance of the screw's blades from the ship, in twin-screw ships especially, would greatly influence the performance. If anybody would take the trouble to refer to Mr. Wright's paper of last year, he would find that the best results were obtaind from screws which worked nearest to the bottom of the ship—two-bladed screws of exactly the same diameter as the four-bladed screws, which were first tried, and which gave unsatisfactory results. I am not saying that we should, therefore put screws as near as possible to the bottom of the ship—I do not mean that; but, when I hear the contrary opinion expressed so forcibly, I wish to draw attention to the facts which are to be found in Mr. Wright's paper.

ON THE PECULIARITIES OF BEHAVIOR OF STEEL PLATES SUPPLIED FOR THE BOILERS OF THE IMPERIAL RUSSIAN YACHT "LIVADIA."

By W. Parker, Esq., *Chief Engineer Surveyor of Lloyd's Register, Member of Council.*

[Read at the twenty-second session of the Institution of Naval Architects, April 6, 1881, the Right Hon. the Earl of Ravensworth, president, in the chair.]

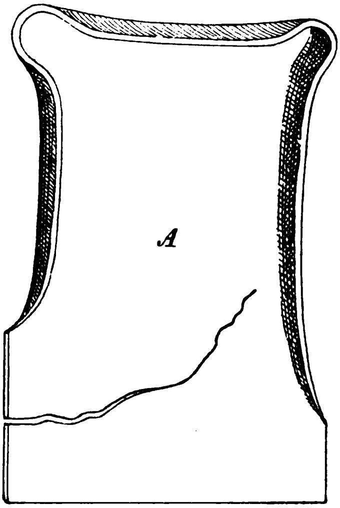
At the annual meeting of this Institution in 1878 I had the honor to read a paper on the "Use of mild steel for marine boilers," and I then ventured to shadow forth that when greater care and attention came to be bestowed by our workmen in' the manipulation of this material, it would to a great extent, if not altogether, take the place of iron for such purposes. At that time there had been only two marine boilers of the modern form made of mild steel, but the manifest advantages this material offered over iron rendered its extensive application evident. Its great strength enabled a reduction to be made in thickness to the extent of 25 per cent. from that required in iron, or keeping the same thickness, an increase of pressure of 33 per cent. Its great ductility and general uniformity had also, it will be remembered, been demonstrated by innumerable experiments embodied in papers read before this and kindred institutions.

A few statistics on the progress of the use of steel for marine boilers since the date above mentioned may not be without interest. Within the twelve months subsequent to that date 120 steamships had been fitted with boilers made of this material, and during the same period in the following year, 280 vessels had thus been fitted, representing over 600 boilers, while up to the present time no less than 1,100 steel boilers have been constructed, and are now in use in steamships, representing a total weight of over 17,000 tons of material.

In the days of its early application two points remained open to doubt by many people, and were the cause of some hesitation as to the development of steel in this direction. One related to corrosion, which does not properly come within the scope of this paper, and the other related to the possibility of brittle steel plates finding their way into boilers, and thereby becoming a source of danger. On the latter point I may state at once, on my own experience, and on the authority of all the engineer surveyors of the society to which I have the honor to belong, that not one single instance of a *brittle* steel plate has come under our notice during the manipulation of these 17,000 tons of steel, and it was not until the peculiar behavior of the plates of the *Livadia's* boilers occurred that we had any fear on the score of brittle plates.

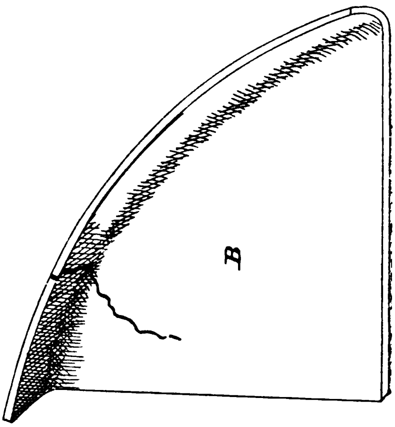
During the construction of these boilers, and especially during the first year, we often heard of mysterious fractures in steel plates which had stood all the tests required, been riveted up into their places, and had then, as it was termed, *cracked without being touched.* A great number of these cases have been investigated, and in every instance they have been clearly traceable to improper manipulation of the material, and the plates in the vicinity of the fracture were found to be perfectly ductile after the fracture had relieved the strain upon the material. These cases were quite as numerous in steel manufactured by the Siemens process as by the Bessemer process. Figures A and B will show two plates which have thus torn, and which had been locally heated for the purpose of flanging.

SIEMENS STEEL.



Tube plate of a marine boiler.

BESSEMER STEEL.



Back end of a marine boiler.

Fractures of this kind at one time used to cause some anxiety, and they have been over and over discussed at meetings of this and other institutions. They were clearly shown to be due to internal stresses set up in the plates by improper manipulation, and under certain conditions a tearing action was set up. As Dr. Siemens put it at the meetings of the Iron and Steel Institute in 1879: "A tear meant applying the whole of the force available upon absolutely one point of the metal. At that one point the metal might be capable of stretching over 50 per cent., but this would not save it, inasmuch as the force was confined to a point. At that point it would tear, and afterwards at another point, and so on. All these mysterious fractures reported might in nearly all cases be attributed to tearing action, which was generally set up at rivet holes, if the rivets at those holes took more than their due proportion of the strain." Another illustration given by Dr. Siemens was that of a strip of India rubber slightly nipped with a pair of scissors, and then stretched, when it would tear right across from the point of discontinuity.

Now, I may state at once that the fractures of the steel used in the boilers of the *Livadia* are entirely different from anything we had previously had experience of, and, what is more, we have still later experience of other steel behaving in the same way, and I think the time has come when steel makers must sift the matter to the bottom, and not only find out the cause, but find out the remedy.

When it became rumored that the steel used by Messrs. John Elder & Co. in the construction of the boilers for this vessel had given way while the boilers were under hydraulic test, I obtained permission to investigate the matter as thoroughly as I could, as it was felt that unless the reason of the failure in this particular instance was ascertained, the uncertainty as to its cause might not only retard the more extended use of mild steel, but would throw considerable doubt upon the safety and efficiency of boilers already constructed of this material.

Messrs. John Elder & Co. kindly afforded me an opportunity of inspecting the boilers which had ruptured, and in addition to this they very willingly gave the fullest information in their power bearing upon the case, and were good enough to send to London a large portion of the plates which had actually fractured, so that experiments might be made thereon. I also had an interview with Messrs. Charles Cammell & Co., of Sheffield, the manufacturers of the steel in question, and Mr. Alexander, on the part of that firm, stated that they were unable to account for the failure of the material, and in the interests of every one concerned would like to have the matter fully and impartially investigated.

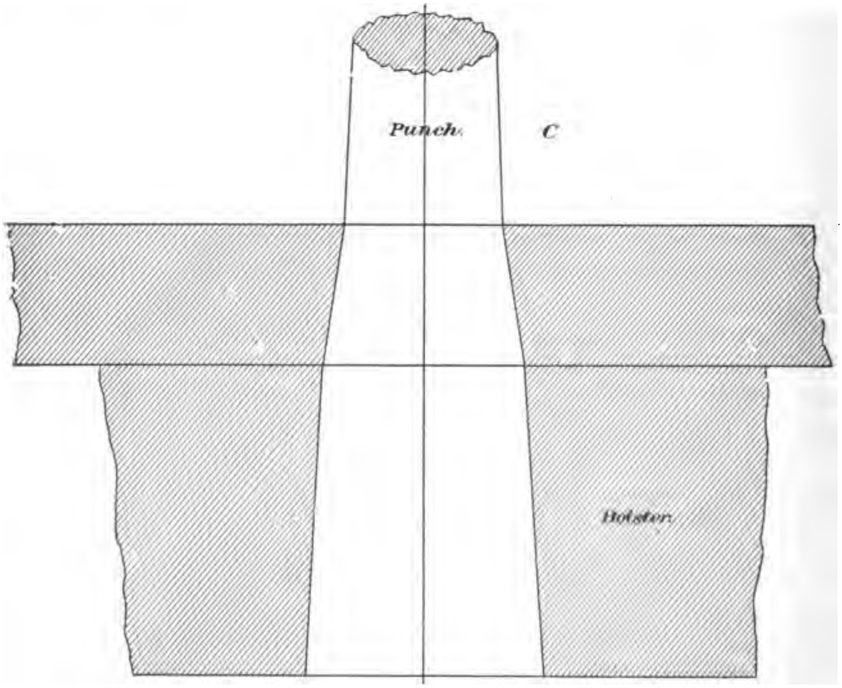
It appears that the plates were specified to be Messrs. Cammell's subcarburized steel, made by the Siemens process, each plate to have a tensile strength of not less than 26 and not more than 30 tons per square inch, with an ultimate elongation of 15 per cent. in a length of 6 inches. In all there were 154 plates supplied, and from these 219 pieces were cut for bending tests; the cold bending and temper tests were made—and were nearly all satisfactory when made—from the rough shearings, and in those cases where the rough sheared pieces broke before bending to the required curve, duplicate test pieces were planed and properly prepared, when they all proved to be satisfactory. There were also 14 tensile tests made at the works, the tensile strength varying between the limits of 26.1 and 28.3 tons per square inch, the elongation in 8 inches varying from 27.3 to 34.3 per cent. The tabulated results of these tests, supplied by Messrs. Elder & Co. and Messrs. Cammell & Co., are set forth in Table A.

On the arrival of the plates at Glasgow, Messrs. Elder, for their own satisfaction, had some check tests made, which practically corroborated those previously made at the steel works. The details of these tests are shown in Table B. It will be seen from these tables that so far as tenacity and ductility are concerned the plates appear to be all that could be desired.

The *Livadia*, it may be mentioned, was intended to be fitted with eight double-ended and two single-ended cylindrical boilers, each of 14 feet 3 inches diameter by 16 feet long and 8 feet 6 inches long, respectively, constructed to work at a pressure of 70 pounds per square inch, the shells being of steel and the internal portions of iron. The shells were $\frac{3}{4}$ inch thick, made in three courses of plating—lap-jointed, treble-riveted in the longitudinal seams, and double-riveted in the circumferential seams, as shown in Fig. 1 appended, and would, I may observe, be eligible, by the rules of Lloyd's Register for determining the safe working pressure in marine boilers, to work at a pressure of 75 pounds per square inch.

Judging from the results of investigations into previous fractures, the first inquiries that would naturally occur to any one were, whether the plates had been punched, whether they had been annealed after punching, whether they had been locally heated; and, if so, whether they had subsequently gone through the process of annealing. On visiting the works of Messrs. Elder & Co., I was informed that the plates were all punched, the holes being about $\frac{1}{16}$ inch less diameter than that of the rivets, then slightly heated and bent to their required curvature, and afterwards put together and the rivet holes rimed out fair to the finished size in place.

Figure C represents (in full size) the punch and bolster used by Messrs. Elder.



I was also informed that while under this treatment one of the plates accidentally fell from the slings onto a piece of metal, which indented it considerably, but did not crack or injure it in the vicinity of the indentation, but it was observed that the plate had cracked at a number of the rivet holes. Messrs. Cammell were in consequence communicated with, and Mr. Alexander, their representative, who examined this plate, gave it as his opinion that the plates had all been injured by punching, and should be properly annealed to restore the material to its normal condition. Accordingly, the plates were all sent to the makers' works at Sheffield, and annealed in a furnace which had been specially altered for this purpose. A sketch of this furnace, and of the position of the plates while being annealed, is appended (*vide* Fig. D, Plate I). The plates were afterwards returned to Glasgow, and riveted up in their places, and the boilers completed without any further cause for alarm.

On subjecting the first boiler to the hydraulic pressure, before the test pressure of 140 pounds per square inch was reached, the boiler shell tore asunder in three places, the cracks appearing to have started amongst the rivet holes of the longitudinal seams, and to have extended at the back of the rivet holes across the plate (*vide* Fig. E, Plate I). In the second boiler, which it was intended to test, the shell plates were found to be cracked in a similar manner behind the rivet holes, *before any water had been put into the boiler*. Mr. Bryce, the manager of the engineering department of Messrs. Elder's works, at once gave instructions to cut the whole of the steel shells from these boilers, and they were replaced by others made of steel manufactured by the Steel Company of Scotland. These plates, which were worked in precisely the same manner, gave no special trouble in working, and the boilers proved to be satisfactory under test.

On examination of the cracked or torn shell plates they were found to be extremely brittle; large pieces were easily broken off in my presence by one blow from an ordinary sledge hammer. In other words, here was a material which had satisfactorily withstood all the mechanical tests recognized by Lloyd's Register, the admiralty, and the board of trade, as sufficient to determine its suitability for the purpose for which it was being used, and was now shown to have become as brittle as cast iron, or even more so, after having apparently undergone only the working usual in boiler making.

Many causes were hastily assigned for the remarkable change the steel had apparently undergone during manipulation, and all more or less plausible. These possible causes were investigated as rigidly as was possible.

1. It was suggested by some that the steel contained too much silicon, sulphur, or phosphorus, or that perhaps the carbon was too high and that the chemical analysis would clear it all up.

2. It was suggested by others that the material could not have been made by the Siemens process, but must have been produced by the Bessemer process, and possibly had been overblown.

3. It was suggested also that the material might have changed its nature during the process of annealing, and that brittleness might have

been induced by the absorption of gases in the annealing furnace, which would be contained in the material in an occluded state; or that perhaps the edges of the plate had been burnt during the process of annealing.

4. It was finally suggested that the cause might be traced to the action of punching or riveting, assuming all the tests that had been made to be *bona fide.*

Accordingly chemical analysis was resorted to as being likely to throw some light on the subject, and I must say that in this instance I was most fortunate in obtaining the co-operation of several of the most eminent metallurgists and chemists in the kingdom, who have kindly furnished me with careful analyses of samples of the material, and have also given me valuable hints and information in the course of this investigation. The results are set forth in Table F, which also contains the analyses previously made for Messrs. Elder by the Glasgow city analyst, together with analyses of steel of different companies' manufacture which has not given trouble in working. Although there are one or two points of difference in the results, they may be said to fairly agree, and they do not show any reason why the material should behave in an anomalous manner; and considering that the raw material used was thus shown to be of suitable quality, it became necessary to look either to the mechanical treatment by which the ingots and slabs were converted into plates, or to the subsequent treatment which the plates had undergone.

When the portion of boiler shells was received in London one of the fractured plates was separated from the other plates by carefully drilling out the rivets connecting them, and several strips were sawn cold out of the failing plate for testing purposes from the positions shown in in Fig. 3. Of these strips two each of 1 inch, 1½ inch, and 2 inches width respectively were tested by Professor Kennedy, of University College, for tensile strength and elongation, with the results shown in Table G, specimens B, C, D, E, F, and G. The results seemed to be everything that could be desired so far as tenacity and elongation are concerned, *but the fractures appeared to be altogether different from those usual with good steel*, the distinct signs of lamination, the peculiar color, and the crystallization being entirely new features.

Other strips about 4 inches wide (Specimens *a*, *b*, *c*, and *d* in Fig. H, Plate I) were taken and subjected to bending tests. They were each found to be extremely brittle in proximity to the rivet holes, breaking across at the holes with one blow from the hammer, while but a short distance from the holes the strips bent cold to the same curvature as would be expected in ordinarily good steel; and on one of the strips being raised to a white heat and allowed to cool, it was bent nearly close without any signs of fracture.

As already stated, the holes in the original boiler shell had been punched and then rimed out slightly to make them fair, and the plates afterwards annealed. As so small an amount was rimed out from them, it would happen that in cases where the holes were unfair at first, the whole of the riming would be taken off one side of the hole, leaving the other side in the same condition as when punched. This is seen in several of the holes from which the rivets have been carefully removed, but as the plates were annealed after punching, previous experience would warrant the supposition that all injury from punching would be thereby removed.

In order to ascertain the effect of punching on the material, several holes were punched in these strips, and without exception it was found that the strips immediately became as brittle as the material in the vicinity of the original holes in the boiler, one blow being in every case sufficient to break them across the breadth of 4 inches, and no perceptible bending being produced before fracture. Pieces were then taken and holes punched therein; one piece was annealed after punching, in another the holes were rimed out one-eighth inch larger in diameter than the large side of the punched holes, and in a third piece the holes were drilled. In each case these samples bent to right angles across the holes without fracture. Similar pieces were prepared from steel made by the Steel Company of Scotland, the Landore Siemens Steel Company, and the Parkhead Steel Company, but in these cases the punching seemed scarcely to affect the bending capabilities of the material, the strips with punched holes bending to the same extent across the holes as the unpunched strips.

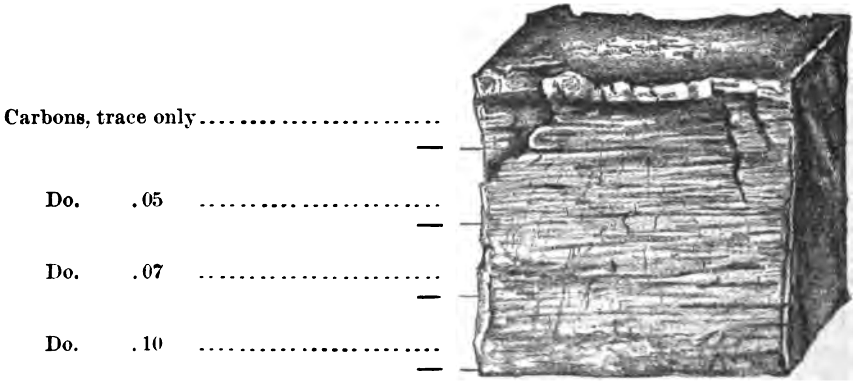
These experiments were afterwards corroborated by cutting off parts of the original joint of the plate from which the rivets had been drilled out, and striking each piece light blows with a hammer, which broke them easily into small pieces; but on the material from the same locality being annealed it was found to bend almost double even across the holes (*vide* Fig. H, Plate I, Specimens *e* and *f*). A portion of the unpunched plate was also taken, and holes punched in it of the same diameter and pitch as those in the boiler seam, and it behaved in a precisely similar manner (*vide* Fig. H, Specimen J). A piece of plate was annealed before the holes were punched; it was also nearly if not quite as brittle as the unannealed piece, and a further piece annealed after punching was found to bend well (*vide* Specimens K and L).

Four strips were then cut diagonally between some of the rivet holes of the boiler, as shown on Fig. H; one of them broke into three pieces with a blow from a hammer whilst trying to straighten it, and a similar piece, after being annealed, bent nearly close without fracture. A third piece was planed parallel, the punch marks being removed, and on being subjected to tensile test it was found to have a tenacity of 29.25 tons per square inch, with an elongation of only 4.7 per cent. in 5 inches (*vide* Table G, Specimen H). The want of elongation observable in this specimen, combined with the curvature after testing, indicated that the stress caused by punching was not entirely removed, and accordingly a fourth strip was planed smaller, the metal being planed off at least one-eighth inch from the edges of the holes. This piece showed a tensile strength of 30.58 tons per square inch, with an extension of 16.8 per cent. in 5 inches (*vide* Table G, Specimen I). The elongation in this piece was more satisfactory and the reduction of area was considerable, but this piece was also curved after testing, showing that even the comparatively large amount planed off did not entirely remove the metal affected by the punching. These experiments conclusively proved that the metal in the vicinity of the rivet holes was considerably injured, being rendered quite brittle, that annealing restored it, and that other steels of apparently the same chemical composition and standing the same mechanical tests were not so affected.

It being thus found that punching holes in the plates made them extremely brittle, some specimens were prepared to ascertain the effect of that operation on the tensile strength of the material. A piece of plate was punched with two rows of holes and then sawn into strips, each strip containing two holes. In one strip the holes were rimed out, another was annealed, and the third was plain punched. On being tested, the rimed specimen broke at a stress of 27.6 tons per square inch, with an elongation in 8 inches of 7.5 per cent.; the annealed specimen stood 26.6 tons per square inch and stretched 6 per cent. in the same length; and the plain punched piece broke short off with no extension under a strain of 18.4 tons per square inch (*vide* Table G, Specimens *g*, *h*, *i*).

These experiments merely corroborate those already published by various authorities on the effect of punching, riming, and annealing steel plates made by either the Bessemer or the Siemens process, namely, that plates above ⅝ of an inch thick punched with the largest diameter of bolster as compared with the diameter of the punch, consistent with obtaining a fair hole, lose about 30 per cent. of their tenacity, while riming out the holes or annealing the plates after punching restores the material to its original strength.

A narrow strip was cut from the plate next to that previously experimented upon, from a part of the plate remote from any of the rivet holes, and it was found to be extremely brittle. The fracture of this piece presented a very peculiar appearance, as will be seen by the following sketch:



The outside sixteenth of an inch on one side presents mirror-like facets from one-eighth to one-quarter inch wide, the rest of the fracture being of a fine granular crystalline, but striated appearance. This outer layer is very soft, being easily nicked or cut on the edges by a penknife, and, as will be observed from the analysis, it contains no carbon, while the layer immediately below this contained a little, the inside of the plate containing more. This fact may perhaps account for the difference between the several analyses.

It would thus appear probable that this part of the plate had lost some of its carbon by a process the inverse of cementation, either by being overheated at one corner while in the form of an ignot or slab, or by a similar overheating while being annealed.

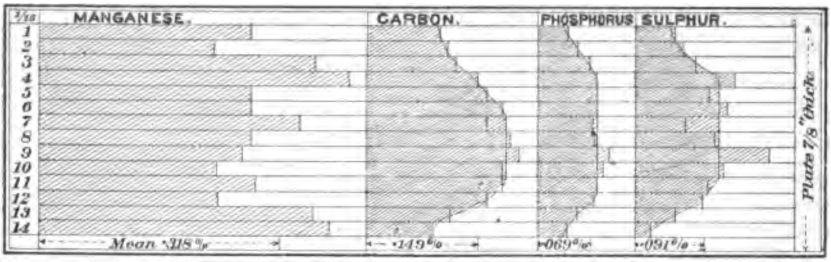
In further confirmation of the foregoing analysis, I have the results of another series of experiments on a piece of these plates from a gentleman of high authority, who has taken the greatest interest in this investigation. He took a portion of the plates about eight inches long and four inches wide, and after having carefully removed all the rust from the outside by grinding, he had successive layers planed off from one side to the other. Each layer was one-sixteenth of an inch in thickness, and the layers were numbered in succession as they were planed off. The results, as will be seen from Table I, are extraordinary. I have also shown the variation of the carbon, phosphorus, and manganese on the annexed diagram.

TABLE I.—*Analysis made by an eminent metallurgist on a sample of the steel which failed in the boilers of the Livadia.*

Shavings about $\frac{1}{16}$ inch thick were taken off in successive layers through the thickness of the plate, and carefully analyzed for manganese, carbon, phosphorus, and sulphur, with the following results:

	Mangan.	Carbon.	Phosphor.	Sulphur.
	Per cent.	*Per cent.*	*Per cent.*	*Per cent.*
1st $\frac{1}{16}$ inch cut	.280	.100	.040	.054
2d $\frac{1}{16}$ inch cut	.230	.110	.054	.065
3d $\frac{1}{16}$ inch cut	.367	.120	.069	.080
4th $\frac{1}{16}$ inch cut	.410	.150	.076	.131
5th $\frac{1}{16}$ inch cut	.283	.160	.077	.097
6th $\frac{1}{16}$ inch cut	.283	.180	.079	.123
7th $\frac{1}{16}$ inch cut	.348	.160	.072	.066
8th $\frac{1}{16}$ inch cut	.381	.190	.074	.105
9th $\frac{1}{16}$ inch cut	.371	.200	.095	.177
10th $\frac{1}{16}$ inch cut	.237	.180	.086	.118
11th $\frac{1}{16}$ inch cut	.288	.180	.079	.098
12th $\frac{1}{16}$ inch cut	.237	.160	.076	.090
13th $\frac{1}{16}$ inch cut	.360	.110	.050	.052
14th $\frac{1}{16}$ inch cut	.381	.090	.039	.018

Distribution of manganese, carbon, phosphorus, and sulphur, in a portion of the Livadia's boiler plate.



I had intended to pursue the question of the possible absorption of gases on this steel, but I regret to say I have been as yet unable to make much progress in that direction. It is pretty clear, however, that there is a fruitful source of inquiry in this direction, which I hope will yet be taken up and pursued by some one more capable of doing so than I can pretend to be.

All the other specimens that were tested for tension presented an entirely different appearance from those of steel that has worked well; some of the fractures were crystalline, whilst in the same fracture other portions were silky, others were filled with distinct laminations and cracks, and the color of the fractures was quite different from the beautiful dark gray so marked in specimens of good steel. The peculiar appearance of the specimens suggested the idea that the plates might have been produced by rolling them from a slightly spongy ingot, the vacuities or air cells in which had been closed up during the operation of hammering and rolling, but the sides not perfectly united. It was also suggested that the plates might not have had sufficient mechanical work upon them, either by being cast in flat ingots and rolled direct into plates, or by cogging the ingots down to the thickness of the slabs instead of hammering them. 'Accordingly a piece cut from the fractured plate was raised to a red heat and rolled to half its original thickness (Fig. H, Plate 1, Specimen X). Strips were then cut from this ⅜-inch plate and punched with holes ½-inch diameter, being one-half the size of those in the ¾-inch plate. This extra work on the material seemed to raise its ductility appreciably, the strips being found to bend well after punching, several of them bending to right angles and only one of them breaking short off, while none of them showed such extraordinary signs of brittleness as were observable in the material when of the original thickness. Three pieces tested for tensile strength after rolling broke under a stress of 33 tons, 34.25 tons, and 32.3 tons per square inch respectively, with an elongation in 8 inches of 12 per cent., 11.25 per cent., and 17.5 per cent., respectively, the last-mentioned specimen being annealed (*vide* Table G, Specimens X). It was also observed that the fractures in these specimens were fine and silky, and had no appearance of lamination or crystallization such as existed before the material received this additional rolling, while the color was precisely the same as that of specimens of steel that has given no trouble in working. In fact, the extra work bestowed upon the plates seemed to render them satisfactory. In order to corroborate these results a second piece was rolled down to the same extent and subjected to similar treatment, with the same results.

The extreme brittleness of these plates, after holes are punched in them, would thoroughly explain the failure of the boiler under hydraulic test, had the plates not been annealed after being punched; but seeing that they were so brittle at the joints when in the boiler and that they became perfectly ductile when re-annealed, it is evident that they could not have been properly annealed in the first instance, after punching. This raises the important question, whether it is possible to anneal very large plates so as to thoroughly set at rest any strains induced by punching or local heating, especially when the plates are bent, and therefore liable to become more intensely heated in one part than in another; and it shows the advisability of drilling all holes in such plates, and so avoiding the necessity for annealing.

The conclusions arrived at from this investigation may be summarized as follows:

I. That there is nothing in the chemical analysis of the material, beyond the want of uniformity described, to account for its behaving in an anomalous manner, and that, so far as can be discovered, the raw material used seems to be of a suitable quality.

II. That the material as first rolled into plates stood the tests required by the specification, and also those prescribed by Lloyd's Register, the admiralty, and the board of trade; the tensile stress, the elongation, and the bending tests being quite satisfactory.

III. That the tensile strength of the material was not reduced by punching to a greater extent than is usual with mild steel, but that the plates when punched—whether one hole in a strip or several holes along the edge of the plate—became *extremely brittle* for some distance from the holes.

IV. That the appearance of the fractures, especially in those samples broken by tension, showed that the material had not had sufficient mechanical work bestowed upon it, a number of minute laminations, cracks, and longitudinal splittings being distinctly visible in the specimens; and this defect, together with punching, made the material entirely unfit for employment.

V. That further working of the material, to the extent required to reduce its thickness to one-half, appears to have nearly, or entirely, eliminated the cause of the brittleness.

VI. That, in order to avoid the use of similar plates in future, the fractures of test pieces should be carefully observed, and should any of them present an unusual appearance, further bending tests should be made on strips having holes punched in them of a diameter about 1¼ times the thickness of the plates.

Since completing the foregoing investigations, and arriving at the conclusions setforth—indeed, since the preparation in great part of this paper—it has been my duty, in company with my colleague, Mr. John, to inquire into another series of failures of plates, this time used for ship-building purposes. The material had been supplied to Messrs. W. Denny & Brothers, Dumbarton, by the West Cumberland Steel Company, Workington, and as some of the plates had fractured in a somewhat alarming manner, we were directed by the committee of Lloyd's Register to investigate the subject.

We were shown a couple of these fractured plates at Messrs. Denny's yard, one being a bow plate, the other a plate from above the boss which cracked through the landing edge. We had a strip about 3¾ inches broad cut from the latter, the strip being in the rough, sheared on one edge and punched on the other. It was placed on an anvil, and when struck with a hammer broke off short. This plate was 15/32 inch thick, and broke at a tensile strength between 31 and 32 tons per square inch, with an elongation varying from 20 to 25 per cent. Having broken this strip off short with a hammer in its rough state, we had its edges planed down to remove those portions injured by the punching and shearing, when it bent over to a radius of ⅞ of an inch without fracture. A ¾-inch hole was then punched in the same piece, and after this it broke again short off with a blow from the hammer.

It was evident from this that the material resembled in its defects the steel which failed in the *Livadia's* boilers. The specimens were sent to the steel works, and we discussed the matter there with Mr. Snelus, who most frankly admitted that the material was not such as it should be, or such as he would knowingly have sent out from his works.

We have no reason whatever to doubt that the material was made by the Siemens process, that the ingots were of a reasonable size, and that they were hammered down into slabs in the usual way. What the exact sizes of the ingots were, however, we could not ascertain, but we were assured that 12 inches was the minimum.

We tested another plate at the works that had been sent back cracked from Messrs. W. Denny & Brothers' yard, and a couple of other plates which Mr. Snelus himself had rejected at the works. Two of these plates were 15/16 inch thick, and had been made for boiler shell plates, and Messrs. Denny's ship plate was 11/16 inch thick. We had shearings cut from these three plates, and in each case they broke off short when it was attempted to bend them cold with their sheared edges in the rough. These plates were all brittle, and it became a question whether they had become so in the process of rolling, either by being rolled too hot or too cold; or whether it arose from an insufficient amount of work having been put upon them. To test this it was arranged to cut the plates up into six equal parts each, and to roll them down in duplicate at three distinct heats. One set was rolled down at the ordinary temperature, another at nearly a white heat, and the other set at as low a temperature as it was safe to adopt without breaking the rolls.

We then cut strips from these reduced plates for testing, and the results may be briefly summed up as follows:

The thick boiler plates $\frac{15}{16}$ inch which were rolled down to $\frac{5}{16}$ and $\frac{6}{16}$ were found to have become ductile, and the rough shearings were capable of performing good bending tests, whether rolled down at an ordinary heat, at a very high temperature, or at a comparatively low temperature, near a black heat. The plates rolled down at the ordinary temperatures, it is true, stood the tests best, but it was evident that the theory that a slight variation in the temperature, such as would be at all likely to occur during rolling in practice, would not account for the remarkable behavior of the steel we were investigating.

In the case of the plate returned from Messrs. W. Denny & Brothers' yard, the effect of the rolling down was not so marked, but this might be accounted for in some measure by the fact that the plate was only $\frac{11}{16}$ thick to start with, and consequently did not get so much extra work put upon it as the two $\frac{15}{16}$ plates did.

Further tests were made in duplicate, all tending to show that with more work on the plates they improved, and that this was the case in spite of great variations in the temperature during rolling. The experiments also showed that when the plates were so rolled down they did not break off short when it was attempted to bend them across punched holes. In fact they bent to a considerable angle before showing signs of fracture at the holes, which then opened gradually, instead of going suddenly right across. I regret to say that we have since heard of further failures of this material in the same yard.

How far the greater or less amount of work done on plates in reducing them from the ingots is accountable for the failures and peculiarities which have recently come to light is a question of the utmost moment, and it is one which to my mind should be thoroughly investigated. I am aware that Mr. Webb, of Crewe, laid it down some years ago that the ingot should be at least twenty times as thick as the plate to be produced from it, and he had doubtless sound reason and experience to guide him in his decision. Whether, however, an inch steel plate rolled from a 24-inch ingot would be in the same condition as a half-inch plate rolled from a 12-inch ingot, is a question I am by no means sure upon. In more than one case during a recent visit to a number of steel works I had the pleasure to hear manufacturers express their perfect readiness to incur considerable expense in experiments rather than leave this question in its present unsettled state. I hope that feeling will become general. The remarkable variation in the distribution of the carbon, phosphorus, and other constituent elements is a point that must also be accounted for, and its true significance ascertained. And I would further add that the testing should be of a more varied and elastic nature than at present. The appearance of the fractures should be carefully watched, and where any departures from the well-known silky fracture common to good mild steel appears, the plates should be subjected to further bending tests on strips in their rough sheared condition, with holes punched in them, and in any other way that hereafter may be shown to be efficacious in discovering untrustworthy material.

I may add that in the case of most of the experiments I have described I have had the advantage of check tests almost in duplicate, which Mr. Kirk, of Messrs. R. Napier & Sons, has been good enough to make, and I have been fortunate enough to secure further check tests in other directions, so that I hope I may say with confidence that the results I have the honor to lay before the meeting are by no means isolated and unrepresentative ones.

In conclusion, I have to express my best thanks to Messrs. J. Elder & Co., Messrs. Cammel & Co., Dr. Siemens, Mr. T. Vickers, Messrs. W Denny & Bros., Mr. Snelus, Professor Kennedy, and many others who have rendered me most cordial and ready assistance in these investigations, and I must express my hope that instead of tending to create a prejudice against mild steel for constructive purposes, the facts I have laid before the meeting will tend rather towards a further improvement in its quality, and towards the removal of one of its last drawbacks, viz, its occasional, and not yet sufficiently understood, liability to fracture in a capricious and unexpected manner, under certain circumstances, while under others it is capable of being bent double cold, and showing remarkable signs of ductility.

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t.					
Tempered.	Cyclops number.	C	per inch ginal tons.	Elongation in 8 inches.	Percentage of elongation.
∠—	268	∧-	28.1	2.75	34.4
∧—	269	∠-	27.5	2.37	29.7
∠—	270	∧-			
∧—	271	∠-			
∠—	272	∧-	26.1	2.40	30.0
∧—	273	∠-	26.8	2.68	33.5
∠	274	∧-			
∧	275	∠-			
∠	276	∧-	26.9	2.25	28.1
∧—	277	∠-	27.1	2.37	29.7
∠—	278	∧-			
∧—	279	∠-			
∠—	280	∧-	26.8	2.19	27.3
∧-	281	∠-	27.0	2.44	30.5
∠—	282	∧-			
∧—	283	∠-			
∠—	284	∧-	28.3	2.50	31.2
∧—	285	∠-	27.9	2.44	30.5
∠—	286	∧-			
∧—	287	∠-			
∠—	288	∧-	28.1	2.37	29.7
∧—	289	∠-	27.7	2.19	27.3
∠—	290	∧-			
∧—	291	∠-			
∠—	292	∧-	27.8	2.31	28.9
∧—	293	∠-	28.1	2.25	28.1
∠—	294	∧-			
∧—	295	∠-			

TABLE B.—*Check tests made by Messrs. J. Elder & Co., on receipt of the plates.*

[Besides these tensile tests, a number of strips were cut and bent cold to corroborate test in Table I with satisfactory results.]

	Test marked.	Direction of rolling.	Dimension of sample.			Strain of section in tons.	Strain per square inch in tons.	Elongation in 8 inches.	Percentage of elongation in 8 inches.
			Breadth.	Thickness.	Area.				
Boiler No. 1....	1 A	Crossways......	.96	.7	.672	18.0	26.78	2.08	26
	1 B	......do..........	.99	.71	.703	19.0	27.02	2.01	25
	1 C	Lengthways....	.965	.705	.68	18.4	27.05	2.01	25
	1 D	......do..........	.965	.71	.685	18.85	27.5	1.88	23.5
Boiler No. 2....	2 A	Crossways......	.98	.52	.509	13.75	27.01	1.88	23.5
	2 B	......do..........	.98	.52	.509	13.6	26.719	1.87	23.5
	2 C	Lengthways....	.98	.53	.519	13.9	26.78	2.0	25
	2 D	......do..........	.99	.53	.519	14.2	27.36	2.34	29.25
Boiler No. 3....	3 A	Crossways......	.97	.54	.523	14.9	28.49	1.86	23.25
	3 B	......do..........	.98	.55	.539	15.3	28.39	2.0	25
	3 C	Lengthways...	.98	.56	.548	15.5	28.28	1.95	24.4
	3 D	......do..........	.99	.56	.554	15.9	28.7	2.15	26.9
Boiler No. 4....	4 A	Crossways......	.985	.695	.684	18.25	26.68	1.97	24.6
	4 B	......do..........	.985	.695	.684	18.45	26.97	2.2	27.5
	4 C	Lengthways....	.99	.69	.683	18.1	26.5	2.29	28.3
	4 D	......do..........	.985	.69	.679	18.15	26.73	2.175	27.2
Boiler No. 5....	5 A	Crossways......	.98	.54	.529	14.85	28.07	2.17	27.1
	5 B	Lengthways....	.98	.54	.529	14.9	28.16	2.05	25.6
	G 1	....	.99	.48	.4752	15.0	31.56	1.78	22.2
	G 2	....	.99	.48	.4752	15.3	32.19	2.0	25.0
	G 3	....	.99	.47	.4653	15.2	32.66	1.86	23.2
	G 4	....	.99	.47	.4653	15.2	32.66	1.84	23.0
	1	....	.99	.49	.4851	15.3	31.53	1.75	21.9
	2	....	.99	.49	.4851	15.2	31.33	1.73	21.6
	3	....	.99	.49	.4851	15.3	31.53	1.73	21.6
	4	....	.99	.49	.4851	15.2	31.33	1.73	21.6

Tests marked G were heated and allowed to cool in ashes.

TABLE F.—*Results of analyses made by six eminent metallurgists on samples of the steel plates which failed in the boilers of the Livadia, and of analyses made on samples of good boiler steel.*

	Livadia boiler steel.						Good workable steel.			
	A.	B.	C.	D.	E.	F.	G.	H.	I.	K.
Carbon..........	.063	.077	.100	.120	.125	.127	.104	.140	.160	.165
Manganese.......	.543	.640	.432	.420	.662	.552	.290	.641	.304	.684
Sulphur..........	.064	.092	.080	....	.073	.094	.064	.056	.048	.084
Phosphorous.....	.063	.064	.045	.063	.060	.050	. 70	....	.055	.060
Silicon...........	.015	.046	.063	.028	.020	trace	trace	....	....	trace

Analysis of a sample of steel plate received on the 2d instant from Messrs. John Elder & Co., Fairfield, Govan.

	Per cent.
Iron	99.116
Carbon, combined	.130
Carbon, graphite	absent
Manganese	.600
Sulphur	.082
Phosphorous	.072
Silicon	mere trace
	100.00

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Analytical and Consulting Chemist.

CITY ANALYST'S LABORATORY,
138 *Bath street, Glasgow, June* 4, 1880.

TABLE G.—*Report on tests conducted by Professor Kennedy in the engineering laboratory, University College, on samples of steel cut from the boiler plates of the Imperial Russian yacht, Livadia.*

Date.	U. C. L. Test No.	Dimensions.			Limit of elasticity.	Breaking load.	Percentage of extension.	Position in plate. (See Fig. 2.)	Remarks.
		Breadth.	Thickness.	Area.					
1880.		*Inch.*	*Inch.*	*Sq. inch.*	*Tons. Sq. inch.*	*Tons. Sq. inch.*			
July 28	469	0.672	0.667	0.448	17.9	29.4	19.8 in 3 inches ..	A	Uniform finely granular "cup" fracture, with silky luster.
Sept. 2	485	0.997	0.744	0.742	17.48	29.48	21.6 in 8 inches *. 26.5 in 6 inches...	B	Fracture, about 5 per cent. crystalline, the rest silky. Lamination distinct, with longitudinal splitting, and distinct appearance of defective weld. (Scale removed before testing, one side only, by hammering and rough filing.
	486	1.000	0.742	0.742	18.2	29.75	20.2 in 8 inches *. 23.0 in 6 inches...	C	Fracture silky, slightly specked with crystal. Lamination quite distinct, and traces of splitting. (Scale removed as No. 485 before testing.)
	487	1.499	0.659	0.988	18.15	29.82	19.5 in 8 inches...	D	Fracture silky, specked with crystal. Lamination very distinct. A pale, salmon-red color is very distinctly noticeable over a part of the fracture, but only in one of the two broken ends. (Planed all over one side to take out a flaw.,
	488	1.497	0.742	1.111	15.43	29.86	18.9 in 8 inches *. 22.0 in 6 inches...	E	Fracture about 75 per cent. crystalline, and very distinct lamination throughout. (Scale removed as No. 485 before testing.)
	489	1.994	0.656	1.308	16.93	29.79	23.9 in 8 inches *. 28.2 in 6 inches...	F	Fracture, about 20 per cent. crystalline, remainder silky. Lamination very distinct. (Planed all over one side, as No. 487.)
	490	1.994	0.656	1.308	16.04	30.20	23.1 in 8 inches *. 28.0 in 6 inches...	G	Fracture, about 30 per cent. crystalline, remainder silky. Lamination very distinct. (Planed all over one side as No. 487.)
Sept. 3	491	0.742	0.674	0.500	18.93	29.25	4.68 in 5 inches ..	H	This piece was of the form sketched in Fig. 3, cut obliquely across plate between rivet holes. It was machined at the sides to remove all injury or marks from rivet holes, which reduced it to the dimensions given. † Fracture quite square across, and entirely (and somewhat largely) crystalline.

* As the specimens were made, the 8-inch length was between shoulders, and included fillets of ⅜-inch radius at each end. The extensions on 6 inches are therefore given in addition.

† It was particularly noticeable that these two pieces pulled themselves into sinuous curves. This indicates to some extent how greatly the material must have been strained by the formation of the rivet holes.

TABLE G.—*Report on tests conducted by Professor Kennedy in the engineering laboratory, University College, &c.*—Continued.

Date.	U. C. L. Test No.	Dimensions.			Limit of elasticity.	Breaking load.	Percentage of extension.	Position in plate. (See Fig. 2.)	Remarks.
		Breadth.	Thickness.	Area.					
Sept. 30	523	0.773	0.500	0.386	18.88	30.58	16.8 in 5 inches...	I	† Fracture granular and silky, lamination visible, and slight splitting. Specks of crystal just visible. Specimen cut diagonally across lap of plate as No. 491 (see Fig. 2). Tested as received, machined all over. Scribed with center line and cross lines ¼ inch apart before testing.
Nov. 1	557	1.245	0.361	0.449	20.38	33.01	11.9 in 8 inches...	X	Fracture of the ordinary " stepped bevel" shape, showing nothing unusual except a very slight trace of longitudinal splitting. The specimen was *not* annealed.
1881. Feb. 14	680	1.242	0.350	0.435	18.26	32.31	17.5 in 8 inches... 29.5 in 2 inches...	X	*Annealed.*—Trace of glassy substance, which looks black through microscope in fracture.
	681	1.272	0.366	0.466	13.41	34.26	11.25 in 8 inches... 27.5 in 2 inches...	X	*Not annealed.* The specimens 557, 680, and 681 were cut from a plate which had been rolled down from ¾ inch to ⅜ inch.

The following tests were conducted by the engineer surveyor to Lloyd's Register at West Hartlepool:

1880. Oct. 27	....	3.75	.75	2.062	....	18.4	none in 8 inches...	g	Two holes, 1 inch diameter *punched* on center line (*not annealed*).
	....	3.75	.75	2.062	....	26.6	6.0 in 8 inches...	h	Two holes 1 inch diameter, *punched* on center line (*annealed*).
	....	3.75	.75	1.875	....	27.6	7.5 in 8 inches...	i	Two holes, 1¼ inches diameter, *drilled* on center line (*not annealed*).

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† It was particularly noticeable that these two pieces pulled themselves into sinuous curves. This indicates to some extent how greatly the material must have been strained by the formation of the rivet holes.

DISCUSSION.

Mr. KIRK. I think that we must all agree with the remark that our very best thanks are due for the liberality with which Messrs. John Elder & Co. and Messrs. Cammel & Co. have placed unreservedly all the facts of the case in the hands of Lloyd's committee, and we must thank them also for the trouble they have taken in investigating the matter. When this question came up, Mr. Parker asked me to allow him to send a sample piece of steel down, and asked that I would try, if possible, and find out the cause of its brittleness, as he was doing the same in London; and when I had made what experiments I could, I suggested that we should compare notes. I acceded to the appeal, and the first thing that came out in trying the stuff was, that by very slight working at the smith's fire a piece of steel, which was so brittle that taking a piece about a foot long with the natural curvature of the boiler something like one-eighth of an inch in a foot, and straightening that in the gentlest method possible, it did not sever itself, but it cracked nearly half through its thickness. The straightening was done by a simple process, without a blow, in the gentlest possible manner. That same piece of steel was taken to the smith's fire and drawn out into a piece five-eighths of an inch square, and you could double it up without any fracture. Then followed certain experiments of rolling it out, as described in the paper, to half its thickness, after which the material would, I think, have passed any tests of Lloyd's committee, and would have stood punching. From this it appeared evident that the material *per se* of which the plate was made (I do not talk of chemical analysis now, but physical tests) was originally good enough, but in the process of becoming a plate there was something very wrong in it; what that was we could not tell. The next idea was, might it have been spoilt by annealing? I took a sample of a good plate. I believe the first samples I tried were portions of H. M. S. *Stanley*, and I tried if I could spoil them to the same extent by any kind of annealing. I could to a certain extent, but not to such a brittle state as these punched plates were in. I found that a good deal depended on how the annealing was done. I have had here the annealing experiments repeated in a better form on pieces cut from the same piece of plate. The piece here was made by the Steel Company of Scotland. This is a piece of the original plate not annealed or done anything to, but doubled up and hammered close together by the steam-hammer, and you will admit that that is a pretty severe test. This is another piece out of the same plate, which was kept twelve hours in the forge furnace during the night with the chimney closed at a full white heat in a reducing flame.

Dr. SIEMENS. Was it a gas furnace?

Mr. KIRK. No; it was an ordinary forge furnace with a coal fire, but the damper was closed, and there was no access of air to the furnace. The next day it was bent close without cracking, as you see it, and by the process of annealing, if you like to call it so, the plate was uninjured. Here are two further examples of the same plate, which were also annealed. This piece was laid in the forge furnace on the Saturday when the fire was drawn. The furnace was at a white heat, and a good current of air was allowed to pass over, until the plate was cooled down. It was then bent, and in hammering close was deeply cracked. This last piece was heated in a boiler-maker's furnace twelve hours, with the door kept slightly up, so as to allow access of air, and it was bent in the same way as the rest, and there is the result—it is broken. By this treatment, although I spoiled the plate, you will see I did not bring it into the state the plates of the *Livadia* were in, and it further shows that much depends on how annealing is done. It may be taken as proved that steel is not spoiled by prolonged heating, even at a white heat, if air is excluded, but that exposure to air at even a red heat will certainly render it brittle, or, as the workmen call it, burnt.

Mr. THORNYCROFT. To my mind there is a feature in the furnace in which these plates were annealed which calls for special attention. If you will examine the drawings we have in the paper, you will find stacked a number of plates one above another, all at the same curve, therefore necessarily inclosing a number of layers of air between the plates. I do not know for what time these plates so stacked were kept in that furnace, but these numerous layers of air would take a long time to pass the heat through, and it seems to me very probable that the plates were not all efficiently annealed. With regard to the *Livadia* steel, it appears that this steel was unusually brittle after punching. It seems to me to be very desirable that if carefully-prepared test pieces of steel that is not perfect may be bent to the required angle, and if when they have angular features they are more likely to show causes of weakness, would it not be desirable to judge more particularly from specimens not so carefully prepared, that is, specimens in the state in which some of these sharp angular features may be found. For instance, in the boilers made the holes were all rimed out, but some rivet holes still retain the injured metal, and were, no doubt, causes of weakness. With regard to the labor bestowed on the steel and the fineness of the quality, I can give this testimony: I know, from experience, that very thin plates are always found to be perfectly tough. If you take a very thin steel plate it is always a tough one, and plates up to $\frac{3}{16}$ of an inch seldom show any signs of brittleness. We are accustomed to work plates $\frac{3}{16}$ of an inch cold, and to flange them when cold to a very considerable curvature, which could not be done in iron plates when hot, except of the very best quality.

Dr. SIEMENS. The behavior of the material composing the boilers of the *Livadia* is so extraordinary, and the result so contradictory, that it is certainly of the greatest interest to have it sifted to the very bottom. When the defective condition of these plates was first discovered, Mr.

Parker kindly sent me a piece of plate broken off short by a hammer. The analysis which I had made showed that the metal was irregular in its composition, and it gave a certain percentage of an element which I do not see represented on that table, and that is silicon. The specimen which I had given me contained certainly a very considerable percentage of silicon.

The PRESIDENT. Could you state the percentage?

Dr. SIEMENS. I can give it in the notes; I think it was about .03 per cent., but unfortunately I have not the analysis with me. Other analyses by eminent chemists have given, I am aware, different results; they have shown no silicon. But if we look upon the interesting table that has been placed before us, I think we have quite sufficient ground given us to account for these differences in the analyses. There we have in the same plate, carbon, differing in the proportion of at least one to two between the outside and the inside of the plate; we have phosphorus, a substance that is not easily oxidized, when combined with metal, distributed in the same irregular manner, and the same with regard to sulphur. Now a question arises, how can that very great difference in those constituents have arisen in the metal? Is it that the metal itself—the ingot—was not composed of the same mixture—that it was imperfectly fused, and imperfectly distributed in the ingot mould; or has the metal, after it has been rolled into plates, been subjected to treatment whereby its chemical nature has been so completely disturbed as would appear from that table? I can hardly imagine that in the same ingot—although there may be some differences of composition found between one portion and another—differences of such extraordinary magnitude could possibly have occurred; I am therefore forced to the conclusion that the metal has been greatly injured in the after process to which it has been subjected. Mr. Kirk has brought before us very interesting experiments indeed, showing how steel of undoubted good quality, when heated injudiciously and heated for a length of time in a furnace containing a large excess of oxygen, can be, what we call in the iron trade, burnt. It would have been very interesting if we could have seen another column there, only I admit it would have been very difficult to ascertain it, showing the amount of oxygen absorbed in the metal. I think it would have been found that the plate near the surface had absorbed a very considerable amount of oxygen, which assisted in making it the brittle, unreliable substance which it became when it was expected to stand its work. It is stated in the paper that every precaution was taken, and that when the plates after punching showed defects, they were taken back to the works to be annealed. I am disposed to take considerable objection to that procedure. Mild boiler steel should bear any amount of rough work in a cold condition without receiving the least injury. If by punching mild steel you lose strength, I consider you have a clear proof that that material is not what it should be. So far from diminishing its strength, the very act of punching should increase the strength of the material. In fact, any treatment to which mild steel is subjected, straightening or punching it, or straining it in any one place, so as to alter slightly its form the effect invariably is increase of strength, and not a slight increase of strength either. It is therefore a practice with which I have little sympathy, that of annealing plates after punching. I would prefer to see the metal used, after it has been punched, without being annealed, and if it is a proper metal it should not require annealing, except in case it had been partially heated in a smith's fire. If the holes are afterwards bored to make the edges smooth, that may be desirable to give more elegant work, but for the mere sake of getting strong and reliable work, I do not believe that it is necessary. Punching alone should give you the full strength of the metal when the work is put together. Therefore what we should know, in order to judge this question more fully, is the exact mode in which the metal was treated after it had been made, what sized ingots were cast, and were those ingots hammered before they were rolled; if they were reheated, as they were undoubtedly, what kind of furnace was used, and were they allowed to remain, as sometimes happens, a night in the furnaces. Then, again, Mr. Thornycroft has already alluded to the processes of annealing which are sometimes adopted. I think there is great danger in annealing steel plates: only if a steel plate has been heated in the forge fire, if it has been heated partially in order to bend it, annealing is necessary, to put it as a whole in a natural condition; but if the metal has only been worked cold, I believe it would be much better to leave it unannealed. Then, again, annealing is not always done in a proper and judicious manner; if one plate only could be put into a well-heated furnace, to take up rapidly the temperature necessary in order to put the particles at rest towards one another, and then could be taken out and allowed slowly to cool, probably no danger would arise. But plates are heaped up in the annealing furnace, and in order to bring the heat down to the bottommost plate, a strong flame is applied to the uppermost, and it is but natural that those plates are put in great danger. The probability is that some of them will be overheated, and that others will be heated all round at the edges, but will not be uniformly heated. Such a process of annealing must be, at any rate, fraught with danger to the material produced. Now one word, my lord, with regard to the process used. It has been said here, that the plates last alluded to, those made at the Cumberland Works, were undoubtedly made by the Siemens process. Now, no doubt this statement has been received authentically, but was there not perhaps some little mistake, inasmuch as those works have only lately put up one furnace according to my system, and I was only told the other day by Mr. Snelus himself that it has not yet been properly put to work. I do not, of course, wish to imply that this could not have happened with steel manufactured by one process or another.

Mr. JOHN. I would only say that Mr. Snelus is my authority.

Mr. **Kirk**. The piece I showed you bent there was twelve hours at melting heat without oxygen.

Mr. **Denny**. My lord, as a member of the firm referred to in the latter part of this paper, I hope you will permit me a few words upon the subject. I would refer to Dr. Siemens' remark as to its being doubtful whether this steel was made by the Siemens process. I am unable to say whether it was made by the Siemens or by the Bessemer process. My firm wrote to the West Cumberland Steel Company regarding the plates in question, and they received the answer that some of the plates were made by the Bessemer process and some by the Siemens process; but there was no definite answer as to which plates were made by either process. Before leaving Dr. Siemens' remarks, I may say that I most heartily concur with him that the responsibility of making bad steel must be thrown entirely on the steel-maker. Steel-makers must be their own most severe testers and critics if they wish for prosperity in business. I cannot, however, agree with Dr. Siemens in the idea that working mild steel improves it; we have had no such evidence in our experience, and it would require very strong evidence indeed to convince me of that.

Regarding the peculiarities of this steel, I can only confirm the remarks of Mr. John. The plate shown there was a plate from the vessel we are building, had been screwed up in its place, and the morning after it was screwed up, without any work, it cracked in the manner shown, being simply held together at the upper edge by a small neck of metal; the night was not specially cold. The mishaps referred to have been quite unprecedented. Previously to their occurrence we used 10,000 tons of steel, and found no more serious mishaps than those referred to in my paper of last year. Tests made at the steel works should be more crucial. The tests up to date have been evidently too mild. They have been too much helped by planing and filing and other assistance, which was no doubt done in a fair spirit, because we were all anxious to give the material and maker fair play. Now, we have had our eyes opened to the fact that the tests should correspond as closely to working condition as possible. The society of Lloyd's have lately, and wisely, decided to do their testing at the works because there the tests can be made more severe without interfering with the commercial interests involved. As to confidence in steel, I do not think the mishaps which have happened should in any way damage the confidence in steel. They are isolated, they are easily detected, and will, I am convinced, by Lloyd's society be detected in future, because greater severity will be insisted on in the tests, and no doubt an increased body of surveyors will be employed in carrying them out. They have not shaken our confidence in the material at all, and I may say this, that the owners of this steamer in which the mishaps occurred told me only yesterday they were determined to build every steamer in future of steel. This shows an amount of real confidence. Besides this, if you look around and think of the 1,100 steel boilers referred to in Mr. Parker's paper, working under conditions of great strain with perfect safety, without one serious mishap, I think your confidence will be restored, and that you will agree with the views expressed in this paper, that these mishaps, instead of lessening our confidence, will open our eyes further and enlighten our minds more, and make us more determined to deal with the difficulties before us.

Mr. Riley. I think that the last remark made by Mr. Denny is undoubtedly a just one; that whilst attention is called so strongly to this failure, it is right also that it should be directed to the 1,100 boilers and the 17,000 tons of boiler steel where there has been no such mishap. It is rather singular that we should have brought before us to-day two such instances as the one which happened with regard to the *Livadia's* boilers, and the other with regard to the West Cumberland steel. It is not pleasant to talk of the manufactures of a competing firm; but does not the experience with the steel from the West Cumberland Works interpret the failure of the former steel? Although it is the case that some of the steel sent to Mr. Denny from the West Cumberland Works was made by the Siemens process, I believe it was the first steel made by that process by that firm, and made before they had properly experienced workmen in the works, and before all the apparatus was perfectly complete. Mr. Snelus, we are told, himself admitted frankly that the steel was not such as he would have sent out had he seen it. The variations shown in the composition of these plates before us is very considerable, but, according to what I have heard of the other steel, they far exceed what is placed before us to-day. Dr. Siemens has made reference to the silicon in the steel before us. I noticed that also; but there is another point to be referred to, and that is the effect of the quality of the iron used in making the steel. If the iron charged into the furnace has an excess of silicon in its composition, undoubtedly the steel resulting will not be what is expected, even though silicon is not found present in the steel to a very large extent. That is our experience, and it is a tolerably wide one.

Mr. West. About 18,000 or 20,000 tons of steel plates and bars have passed through the hands of the surveyors of the underwriters' registry for iron vessels since steel made by the Siemens-Martin process was brought into use for mercantile ship-building, and I may say that, with very trifling exceptions, we have had no failures. I may further say, that fully half of it has been steel of the highest tension, which I had the honor of advocating here last year; this has equally well stood all our tests and the practical working in the ship-builders' yards. In ordinary ship-building the steel must be punched, and riming out punched holes cannot be seriously entertained, so that the practical outcome is, that material suitable for ship-building must also be suitable for punching. On this account I was very glad indeed to hear Dr. Siemens say that punching does not injure the quality of good mild steel. I am very much of the same opinion. In the year 1868, when we were testing some Bessemer steel to ascertain to what extent it was injured by punching, we found that in some samples the tensile strength was positively increased by this treatment. I would also mention another set of experiments made at that time: some samples of steel were bent cold through various angles up to 90°, then straightened cold, and afterwards tested for tension; and it was found in these cases also that the tensile resistance was increased. In the case of the *Livadia's* plates, I am afraid that the original testing must have been somewhat perfunctory, and, if so, it is quite easy to understand that a material which was not in all respects perfect might get into the hands of the ship-builder. There is a sentence in the paper which says, "But it was observed that the plate had cracked at a number of the rivet holes." I think if any plate was cracked, sending it to be annealed would hardly put it right. Under such circumstances, annealing seems to me very like shutting the stable door when the horse is gone. Mr. John added a passing remark to this paper to the effect that the plates were made from 22-inch ingots. Surely that is a sufficiently large ingot to insure the necessary amount of working to make a good seven-eighths of an inch plate. If not, what sized ingot shall we require?

Mr. J. D'AGUILAR SAUMDA, M. P. I cannot go the length of the last speaker, and suppose that there is no virtue in annealing under certain circumstances. It is very odd that that which is put forward in the very point which is referred to, has come within my own experience as having resulted in exceptional cases with regard to steel. In exceptional cases I would say that in certainly ninety-nine cases out of a hundred we have now and then found a plate which was hard, and which was incapable of bearing the ordinary tests and working processes which we use, but in those very few cases we have found, although the plate would break under the blow of a hammer, that it has been entirely restored to its natural state of ductility when it has been properly annealed. Therefore, under certain circumstances, there are advantages in regard to annealing. But I quite agree with Dr. Siemens, and, as a general rule, we never use annealing at all; wherever we work cold we never use annealing, and in the cases where we do anneal, they are just those he has referred to, namely, where we have had to pass the steel through forges, such as welding ends into beams, and restoring the general disturbance arising from passing it into the forge fire. I happened to supply to Mr. Ravenhill, and I am told by him that he has attached it as an appendix to his paper, the history of a number of vessels built over twenty years, by myself, of different descriptions of steel—some of cast steel, some puddled steel, some Bessemer process, and some the present process; and those vessels are in every case, without exception, although many of them have been from fifteen to twenty years at work, going at this moment. Those vessels have been run into and have twisted their noses completely round, and they have been all found to do well, and they have not been troubled with any of those chronic diseases described in the boilers of the *Livadia.* I do not pretend, of course, to give any authoritative view, or to discuss at all from an analytical point of view, the construction of the steel used in the *Livadia's* boilers; but all I can say is that, as a general rule, it must have been totally different from that which we have received from the Landore Company, or the Steel Company of Scotland, or from that great manufacturer of steel in Germany, Mr. Krupp, all of which we submitted, without any difference whatever, to precisely the same tests, and, as a rule, it all came up to the very highest requirements and reliability which one could desire to have in any material. I hope the result of this investigation by Lloyd's, the conclusions they have drawn from it, and the general observations we have heard in this room, will go far to strengthen the conviction that, while it is absolutely important that no one should recede in the smallest degree from acquiring the greatest amount of perfection the steel-makers can give, and not allow price to interfere with their desire to obtain the very best quality, we are on the road to introduce generally this material very much indeed in exchange for the inferior material that all of us have been obliged to put up with to the present time.

Mr. MARTELL. I agree with the remarks made by many speakers, that we have a material in mild steel superior in every respect to what we can hope to get from iron. It has greater rigidity, greater ductility, and greater strength, and is in the main uniformly to be relied on; still, notwithstanding this, we should not lose sight of such occurrences as those brought forward in this paper. There is evidently something here in the production of this steel that has caused the confidence of those who use it as ship-owners and ship-builders to be shaken to some extent with regard to the uniform quality of this material. There is a mystery about it. Although mild steel can be relied on generally, we occasionally find there is some kind of mystery in it with regard to its true quality that does not occur in iron, and where this arises it must be closely looked into, and steel-makers for their own sake must endeavor to probe this to the bottom and find out what the cause of it is. I know one steel manufacturer of great eminence who tried himself at one time to roll plates from thin slabs without hammering them down. He found that his men could not do this, and he had to come back to the large ingots afterwards. It is desirable to adopt economical modes such as these if you can produce the best quality of material, but at present it appears you cannot do this. When he found fault with his men, his foreman told him that it was impossible with thin slabs to roll reliable plates of this thickness. Mr. Webb, of Crewe, a man of large experience as a steel-maker, and who thoroughly knew what he was talking about when he said he could not produce an inch plate with less than a 24-inch ingot, spoke with great experience before expressing such an opinion as that, and other steel manufacturers would do well to follow that example, and not try to produce for cheapness plates with less work on them, at least until they can absolutely rely on its quality, especially when they know and have proved by adopting the somewhat more expensive process the material can be depended upon as a reliable article. It is very evident from the experiments which have been made in rolling and hammering that working does improve this material immensely. Something was said here to-day which struck me with astonishment, and, coming from so high an authority, I am bound to pay every deference to it. I have heard some things to-day which are contrary to all our experience both with regard to iron and steel, and I cannot but think the producers of mild steel will only injure themselves by trying to make this material appear better than it is. If the manufacturers of steel attempt to convince us, until they have gone through the actual experience and shown us that steel can be punched and sheared without diminishing its strength, then I think they are attempting to show that which, as far as my experience goes, is impossible for us to accept. We find that another authority goes still further, and says positively that punching increases the strength. Now that is altogether so contrary to our experience, that I should be very sorry for us to delude ourselves because we may have a liking for this material. There cannot be a great deal of expense in producing samples of steel and punching them, and showing us from actual experiments that such samples were not injured by punching. The society to which I have the honor to belong have gone to very great expense, and have tested many samples which have been sent to them. We likewise have had the pleasure of receiving, by the kindness of the admiralty, an account of the samples they have tested, which confirms what we have done, and goes to show that punching this material diminishes the strength of steel as it does of iron. I hope that until we get something confirmatory in the way of practical experience, it will not go forth to the world that steel is not diminished in strength by punching. I say it if for the reason alone, that if ship-builders and ship-owners believe this, the next day we should have very strong representations made with regard to scantlings, and instead of 20 per cent. they would want 30 per cent. taken off from the scantlings required for iron. It is incumbent upon steelmakers to look carefully into it to see what the cause of this failure is, whether it is in the size of the ingots from which the plates are made in hammering them, or from the chemical constituents, or from using inferior iron, or from adopting the direct process without selecting the best kind of iron, or by what means this unreliable material referred to in this excellent paper has been produced. It behooves them to look very carefully into it, and try to restore the confidence in this material, which I must say, whatever may be said to the contrary, has to some extent been shaken. I would confirm what Mr. William Denny has said, that as far as tests go, any new test that will expose the quality of this material should be resorted to. I know that Lloyd's Register

Society will, instead of diminishing the tests, insist that they shall be increased until we can rely on every angle and plate that comes from the steel manufacturer's works.

Sir **Spencer Robinson.** I very much share in the opinion the last speaker has expressed. I do not and I cannot say that I have lost any confidence whatsoever in mild steel being the right material for ship-building. I can bring my little experience in a small way as one of the directors of a ship-building company in the north of England, not one of those referred to by our chairman, a ship-building establishment at Hull. We have built several ships of steel and a great many steel boilers, and we have not had one single case parallel or to be compared to the failures which Mr. John has read to us to-day, but, on the contrary, our success has been complete in every respect. We have had none of those accidents of cracking or going wrong which have been described to-day, and our steel was supplied to us by that company which the noble chairman adverted to, and Mr. Samuda also, the Steel Company of Scotland. The steel coming to us in that way has never failed under any particulars. We have tested it over and over again, and we have made experiments for ourselves in a direction which leads me rather to go counter (which I am very sorry to do) to so high an authority as Dr. Siemens. We have made many experiments in punching steel plates, and we have found invariably a loss of strength in punching, which was in most cases, though not in all, restored by annealing. I know well that ship-builders have to deal with steel which they cannot always do otherwise than treat by punching—they cannot always bore it out. The material of the steel, therefore, which ship-builders use, and which they have a right to ask from manufacturers of steel, is a material which punching shall not affect and shall not weaken. It would be, I think, a very great mistake, and I should be sorry if that opinion went uncontradicted, that the punching of steel is a diminution of its strength, not always restored by annealing. I wish to say in my small way my confidence in steel has not been in the least shaken; that what is certain to restore that confidence is greater pains in the manufacture of steel, and greater care in rolling down the steel plates from a sufficient thickness of slabs; and when that is done, and when the steel is properly worked, then confidence will be restored in it, and in a short time everybody will admit, I hope, that there is no material in the world comparable for ship-builders to the mild steel which can be got in this country.

Mr. **E. A. Cowper.** My lord, I would make just one or two practical remarks in reference to the annealing furnace. The drawing given at the end of the paper of this annealing furnace, I think, would strike every practical mind as being something quite wrong. Many of these plates are $\frac{15}{16}$ of an inch thick. There are eleven shown here, one on top of another. If you can imagine a pile of plates packed as close as they could possibly be, 11, 12, or 13 inches high, the top plate must have a great deal too much heat, or those below not have enough. It is quite impossible in an ordinary annealing furnace to heat such a mass of plates thoroughly and equally through. Some of them will be a great deal too much fired, and others probably not enough.

Mr. C. W. **Merrifield**, F. R. S. My lord, there is a point that I should like to refer to which is mentioned in the paper, and which has come out rather more fully from the discussion; that is, the necessity of completely breaking down, by working it, the structure which the iron acquires when it is brought into the ingot mold. I have seen this in soft as well as hard materials, and my first acquaintance with it was in melting wax when a boy. You must completely break down the structure which it acquires in the casting; that must be completely broken down by actual mechanical work, either by rolling or hammering, before you can depend on the material as being capable of being freely molded in the way mild steel or iron has to be when it is subjected to punching. It is the same thing with wax, lead, copper, and steel.

Mr. W. H. **White**. My lord, I am very sorry that Mr. Barnaby should have left, because he intended to say a word or two about the admiralty experiments with steel, but in his absence I propose to make one or two statements bearing more especially on the paper. Great stress has been laid on the effect produced on the quality of the steel by variations in the amount of work put upon it in the manufacture. I remember one case where a contract was taken for some thick deck armor plates by a firm that had not made such plates before, and the plates were to be made by the manufacturer at a low rate. He attempted to roll them from cast-steel slabs without success. One of those plates, while being simply straightened to go on a deck with a small curvature, broke clean off across its width. It was two inches thick. I have seen other thick plates of that kind which have been broken across a line of holes, on being put through the rolls to straighten them, and on being put through again they have broken again. As Mr. Martell has said, those cases are few and far between. On the other hand, I have met with cases where the work in manufacture has been carried so far that the quality was affected on that account. I have seen steel plates from $\frac{3}{16}$ to $\frac{1}{8}$ thick, which had been worked, some makers would say, too long—which had gone cold in the rolls; anyhow, they had reached the condition of high tensile strength and small ductility, and those same plates when annealed became perfectly satisfactory; the tensile strength was lowered, and the ductility restored. Similar results had been occasionally observed in beams, particularly when turning down the lower part of the beam to weld in the gusset plate. With a beam of 60 or 65 feet long, it has to be put through the rolls a great many times, and it may happen that one end of the beam may get unfair treatment. We have had cases where the large amount of work put on the material has caused extreme brittleness at the beam arm. There, again, I am only speaking of a few cases out of a great many. I quite indorse Mr. Martell's general feeling of confidence in the steel; I am only drawing attention to this fact, that we have, in our experience, known cases where too much work has been put on an ingot in producing long bars and angles, and they have broken off short. Too much work may be an evil rather than a good. It is for the steel-makers to say what is the happy medium that gives us the steel that we want. As regards the effect of punching, I think there is some little confusion; I do not attempt to explain the divergence of opinion between Dr. Siemens and Mr. Martell, but we have made a great many experiments in the dockyards, and Mr. Barnaby has communicated the principal results of those experiments to the Iron and Steel Institute. It cannot be said from our experience, if you start with steel of 28 tons to an inch and punch a hole in a strip, that the remaining steel has 28 tons to the inch.

Dr. SIEMENS. Per square inch of the remaining section.

Mr. WHITE. That is not our experience. It would not be more, taking the different thicknesses, than 26 or 27 tons per square inch of the remaining sections. Our practice is, in important work, in countersinking to go right through and take out the damaged part, and without incurring much additional work we are enabled in that way to get the full strength back again of the remaining section. If you only punch the holes an eighth of an inch smaller than the rivet you intend to use, and enlarge in countersinking, you produce the condition that Dr. Siemens has described. Without this countersinking and riming out you have not that condition. In the experiments made with steel for the navy we have not failed to try what would be the effect of leaving ragged edges. We have tried samples sheared off to shape, others slotted to shape, and others punched to shape, always securing the same effective sectional area. Under direct tensile strain we have not found a difference in the strength of the section, whether it was attained by slotting, punching, or shearing them; but it is very different, indeed, when we come to what are meant to be "standard" forge tests, and I think it is asking too much of the steel that you should shape a piece out in any rough way you like, and deliver blows haphazard upon it, which may involve unequal strains. I think if we are to have accurate results in such forge tests we should always try and test under the same conditions, and that in the admiralty we certainly should do. I quite sympathize with the desirability of thoroughly testing steel, but I think in these standard forge tests we should either bend under a press, or adopt some method so as not to put unfair strains on some parts of the material and have the other parts not suffering. Dr. Siemens's illustration of the slight crack on the edge of a piece of India rubber would come in here. If a man hits down with a heavy sledge hammer close to a crack in a sample even of good steel, it is very likely that he would break it.

Mr. LAIRD. I should like to ask Mr. Denny whether the plate of which he has shown the diagram was heated in order to bend it to the angle required to fit on the stem?

Mr. **Denny**. It was slightly heated.

Mr. **Laird**. I think in that case the failure was most likely due to some alteration in the material from the effect of the heating.

Mr. **Denny**. It was in no way heated differently to some hundreds of bow-plates which have been used in ships in a similar manner.

Mr. **Laird**. In our experience, and we have built many steel ships, there has only been one instance of a plate cracking in a similar manner. In that case it was a garboard strake plate which had been heated in a furnace to bend it to the required angle, and, as in this case, it was cottered up in its place before the crack was observed. I am inclined to think that the heating probably had in this particular instance a great deal to do with the failure of the steel.

Dr. **Siemens**. Might I be allowed to say one word in explanation. There is one observation which I made which has been perhaps rather severely criticised. It concerns a matter of great importance. I do not wish to say whether plates should be punched only and left, or whether it would be better to clean out the holes. But what I say is, that in taking a strip of mild steel (and I refer not to the somewhat harder steel used in ship-building, but to the milder steel used for boilers), and punching a line of holes across a plate of this steel and subjecting it to tensile strain, the total strain that piece of metal will bear would be fully equal, or, in the case of first-class metal, about 10 per cent. superior to the normal strength of the material multiplied in the remaining cross section. I base that view not upon mere theoretical grounds, but upon many experiments which I have made and seen tried, and I should be very happy if Mr. Martell would give me an opportunity of testing it with him.

Mr. J. L. K. **Jamieson**. I think it would be a great mistake, and we should do ourselves very much wrong, if we were to come to the conclusion that we have lost confidence in steel. I boldly affirm, from a long experience of working steel, both in boilers and the plating of ships, that I never came across any material more deserving of the confidence of engineers, ship-builders, and ship-owners than that known as mild steel. You can twist it any direction, and this steel, when it is mild, will bear from 28 to 32 tons tensile strain. Therefore I think that the eminent steel-makers in this room should be able to tell us the reason the steel of the *Livadia's* boilers gave way. I think it is not creditable to themselves if they do not come and tell us the reason. The manner of working the steel used for the *Livadia's* boilers there is no doubt would be the same as that of a great many other boilers made by the firm of John Elder & Co. The usual practice—at least it was when I was a member of the firm—was that the plates were tested at the yard after having been tested at the works. A strip was planed off, both in the longitudinal and the cross section, and carefully tested. The plates were then punched singly in a machine that was capable of punching them with remarkable exactitude, and afterwards the plates were planed to the dimensions required. They were then put in a furnace and heated singly, not as shown in this furnace, as Messrs. Cammell have taken on themselves to do. It was bent in the rollers and allowed to cool in the workshop, and all the material dealt with in that way stood the test remarkably well. This material for the *Livadia's* boilers, having been dealt with in the same way, was in my opinion a different material or differently worked, because it acted differently when it was put in the structure, and I should have been glad that there should have been no doubt about this material. I am sorry that Dr. Siemens and the other eminent gentlemen whom I see around me have not enlightened us on that particular point.

Mr. PEARCE. I have to thank Mr. Parker for introducing this paper to the Institution. In the first place, because it has enabled me to clear up something with regard to this question; and, secondly, because we have had in this discussion certain observations made from which I have gained a great amount of information. I may refer to the question brought up by the last speaker, and say that the method of working the second set of boilers in the *Livadia* was precisely the same as in the first set; and although we were naturally anxious about the second set, I can assure him that there was not the slightest sign of any weakness whatever, showing that this failure was in no way due to the method of working.

Mr. JAMIESON. I did not say that. The working of the steel before it came into Mr. Pearce's hands I referred to, not the working in his works.

Mr. PEARCE. The most important matter that I think has been brought before us is that which was mentioned by Dr. Siemens when he told us that punching does not affect the strength of the material in mild steel, that is to say, the steel that we ought to make boilers of. That is so contrary to all our experience, and we have been in the habit of making tests for all the plates of our boilers which we make, that I cannot accept the statement, although it comes from him on a second occasion. The other question of annealing is also quite contrary to all our experience. We find that if we do not anneal a plate after having worked a portion of that plate, there is a liability to crack. I have had, I may say, during my experience of steel, which has extended over a great number of years now, upwards of a dozen plates crack through the inadvertence of not annealing the whole plate after one end, or a small corner of it, had been put in the furnace or smith's fire. In my opinion, in future all boilers made of steel should be bored and not punched, and the boilers that I am making now are all bored, and, in fact, I may say that the whole of the steel boilers that my firm has made since the *Livadia's* failure have been bored. This failure on the part of the steel of the *Livadia's* boiler has not shaken my confidence in it in the least. I think, looking back to the amount of failures we have had in other materials—I mean iron—certainly for every plate of steel that has failed we have had twenty plates of iron. Therefore why should we, although one batch of plates, as we may term it, have failed, have any doubt as to this material, knowing the other high qualities which it possesses? Now, there is one thing with reference to the fracture of these steel plates that I intended to bring under your notice, and which I do not see mentioned in Mr. Parker's paper. It is this: I notice that when the crack flew across the plate it did not go across in a fair line, in the same way as you usually expect to see a steel plate crack. It went round a certain portion, the same as a knot would appear in a piece of wood, as if the crack was trying to find its way through a tough piece of metal; and I am inclined to think, notwithstanding Dr. Siemens seems to be of a contrary opinion, that it is in the original manufacture of the ingot the mistake is made, and the metal is not all of the same quality, and likely to contain superior as well as inferior metal, and from not being properly worked and amalgamated.

Mr. JOHN. My lord, in the first place I must express my great regret that Mr. Parker is not here to reply on this paper, because he has investigated this matter far more fully than I have, but when a gentleman has temporarily lost the use of his right hand and one eye, he can scarcely be said to be in a position to be present here to-day. In the first place, I should like to offer a few remarks on what fell from Dr. Siemens and Mr. Riley, and before doing so I would express my regret that Mr. Snelus and Mr. Wilson, or some one from Messrs. Cammell's, have not been present here to-day, because they would have thrown light upon some of the matters that are now left in some doubt. Dr. Siemens has drawn a distinction, I think, between the *Livadia's* steel and the Workington steel, and I think his remarks rather went to indicate that what happened to the *Livadia* steel, or what accounts in a measure for the *Livadia* steel failing as it did, might have arisen in the annealing furnace, but that annealing could not have affected the Workington steel. But Dr. Siemens says as to the Workington steel he doubts whether it was made by his process. Now I have it on the authority of Mr. Snelus that it was made by his process; that the boilers he tested there were made by the Siemens process, and there could not have been any misunderstanding about it, because we discussed the thing, and Mr. Snelus dwelt largely on the fact that he had been supplying Bessemer steel for ten years for boilers, because his customers in the shipbuilding world had a preference for the Siemens steel. I have every reason to believe Mr. Snelus, and therefore I cannot see why we ought to draw the distinction which Dr. Siemens appeared to draw between that and this steel, and I do not think that what happened in the annealing furnace accounts for it at all. I believe the Workington steel and this *Livadia* steel is practically the same material, at least it has all the defects, and those defects only become apparent when they are tested in a rough state. For that reason I do not quite agree with what fell from

Mr. White, that we should perform the forge tests and the bending tests on a prepared basis in order to have them all alike. And for this reason, that Mr. Snelus attributes the very fact of these plates going out of his works to this, that instead of trying the bending on the rough-sheared pieces, his manager prepared them, planed their edges, and took the very step to avoid finding out that the plates were bad. There was a curious remark, which I was rather surprised was not noticed, in the paper about what happened with the Cammell steel. There, in the first place, the bending tests were made on the rough specimens; but every plate failed to bend or break when the tests were made on the rough pieces. They planed a specimen as a check test, and did the very thing which prevented their finding out a brittle plate. I cannot help thinking that there is something about these plates which we have not got to the bottom of yet, and I think we want much further investigation into it, and especially as to the want of uniformity. Dr. Siemens has remarked upon the absence of silicon there. Another gentleman remarked upon the very high percentage of silicon in the Cammell steel. I think he said it was .03. It is a very astonishing thing to me that we have been representing mild steel as being uniform right through—homogeneous—and here we find plates with double the amount of carbon at the center of the plates than what they have at the edges. I must say, if that is the state of things that prevails at all largely in steel, that is a circumstance deserving attention. I think there is something in that which really accounts for the whole thing, because the middle of the plate must be quite different from the outside edge. There might be brittleness about one part and ductility about the other, which would throw far greater strains on one part of the plates than on the other, and to my mind almost account for the whole thing. There is only one other point, and that is as to the effect of the work on ingots. Of course, I only have the word of the steel manufacturers as to the sized ingots the *Livadia* steel was made from, and the self same thing applies to the Workington steel. If Mr. Parker were here, he would, I know, wish the meeting, and all who read his paper, to distinguish between the experiments he saw himself and those which he gives on the authority of the makers. Without casting the slightest doubt upon them, I have no reason to doubt that the Cammell steel was made from 22-inch ingots, as stated by them, and that the Workington steel was made from 12-inches and upwards, and hammered. There is the curious fact that if these plates are rolled down to half their thickness they become ductile again, indicating in a great measure it is want of working on them. But against that we have this curious fact, that the old practice of taking out of every cast a small baby ingot and rolling it down for testing purposes rather goes in the contrary direction, because then it could not be reduced to the extent of a twentieth of its thickness, and yet those ingots produced good plates; so that, on the whole, I must confess that after being engaged in this investigation, not so much as Mr. Parker certainly, but, as far as I have gone, I am hopelessly in a fog now as to the real cause of these plates being in this state—a most unfit and dangerous state for ship-building or boiler-making purposes, and I do hope that steel-makers will let us know more about it than either Dr. Siemens or any one has here to-day.

The PRESIDENT. I am quite sure you will allow me to offer to Mr. Parker, the writer of this very valuable paper, your thanks and congratulations upon its ability. I may also point out the value of the extremely interesting discussion which has arisen upon it. When dealing with such a material as steel, it is of inestimable importance that not only ourselves but the public should be made aware, not only of its virtues, but also of its defects, with a view to their correction. We have had the opinions of the makers of steel and the users of steel, and what I may call the inspectors of steel, and there is one most valuable result that has arisen from this discussion, namely, that the confidence of the public, so far as this metal is concerned, has in no way been shaken as to its value for ship-building material. I think I have your permission to offer your thanks to the writer of this paper, and our sincere regret that he is not able, from ill health, to be here to read it himself.

THE INFLUENCE OF CUT-OFF AND LENGTH OF CONNECTING ROD ON THE WORKING OF STEAM ENGINES.

BY C. E. STROMEYER, Esq., *Engineer Surveyor to Lloyd's Register.*

[Read at the twenty-second session of the Institution of Naval Architects, April 7 1881, the Right Hon. the EARL of RAVENSWORTH, president, in the chair.]

In bringing before your notice a few tables which I have compiled on the friction of some of the working parts of an engine and the uniformity of motion, and their relations to the cut-off and length of connecting rod, I hope that the information may be found not without value in the present day, when, as it appears, both the type and the pressure of steam engines may undergo a radical change.

The classification of the different frictions occurring in an engine has seldom been attempted in such a manner as to make apparent their relations to the several dimensions. It will therefore be necessary to submit a few remarks on this subject before explaining the tables and diagrams and the principle on which they have been calculated. The frictions of an engine can be divided as follows:

Those depending only on the dimensions; those which depend chiefly on the weights; and those which depend partly on the dimensions, but chiefly on the adjustments, such as the packing of glands and pistons. Most of this last-mentioned friction is a function of the skill or carelessness of the men who attend to the working, and is therefore inestimable, but there are certain limits below which it can never fall. They depend on the dimensions and nature of the surfaces, and on the lubricants. Nothing, or very little, definite is as yet known about this subject, but it is at least probable that the friction increases both with the initial pressure and with the circumference of the rod or of the piston, and also that the lengths of the packing in the glands and heights of piston rings are important factors, though whether the friction increases directly or inversely as the length, or whether there is a certain length which gives the best result, is not known.

Of the other frictions, that produced by the weights of revolving and sliding machinery can be so easily ascertained for individual cases, that it will not be necessary to refer to it here. Then there is the slide valve friction, which does depend to a great extent on the expansion of the steam, but is entirely independent of the length of connecting rod. The remaining frictions are produced between—1, crosshead and guide bars; 2, gudgeon and crosshead; 3, bottom end and crank pin; and 4, crank shaft and main bearings. As the co-efficient of friction has been pretty accurately determined, and as it is at least certain that it will not greatly vary in different engines in which the same lubricants are used, the above losses can all be brought into formulæ, comparing them with the indicated work and with other engines in which the dimensions have been altered. The following short list contains the notations employed in the formulæ, on the friction and on the uniformity of motion :

s = length of stroke $= 2r =$ twice length of crank.
l = length of connecting rod.
P = load on piston rod
K = normal pressure on guide bars.
Q = thrust in connecting rod.
M = turning moment on shaft.
u = position of top end from top of stroke.
$h, v, x, y,$ fix position of crank pin.
α = angular position of crank.
β and γ = complementary angles to α.
φ = co-efficient of friction.
M = mass of piston and rods.
T = moment of inertia of fly-wheel.
I = moment of inertia of connecting rod.
e = cut off.
I. H. P. = indicated horse-power.
ω = angular speed.
d 2 = diameter of crank pin.
d 3 = diameter of crank shaft.
d 4 = diameter of gudgeon.

Fig. 1.

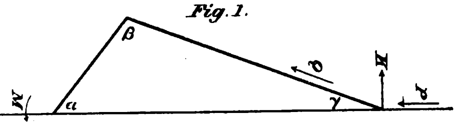
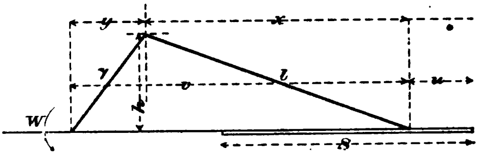


Fig. 2.



It will be seen that—

$$\cos \alpha = \frac{y}{r},$$

and by making several simple substitutions it will be found that—

$$\cos \alpha = \frac{u^2 + r^2 - l^2}{2 v r}.$$

With the help of this formula the values of cos α, α, sin α were tabulated for twelve different values of u and for eleven different values of l, ranging from $\frac{3}{2} r$ to ∞. Then diagrams were constructed representing the friction according to the two following formulæ:

$$\text{Guide friction} \int d f_1 = -\varphi \int \mathrm{K}\, d u = -\varphi \int \mathrm{P} \frac{h}{x} d u.$$

$$\text{Crank-pin friction} \int d f_2 = +\varphi \int \mathrm{Q} \frac{d_2}{2} d\beta = -\varphi \int \mathrm{P} \frac{d_2}{2} \frac{l}{x}\left(1 + \frac{y}{x}\right) d\alpha.$$

The friction at the journals is almost the same as that of the crank pin and the friction of the gudgeon is so very small that they have not been worked into the shape of a table. The formulæ are—

$$\text{Crank-shaft friction} \int d f_3 = -\varphi \int Q \frac{d_3}{2}\, d\,a = -\varphi \int P \frac{d_3}{2} \frac{l}{x}\, d\,a.$$

$$\text{Gudgeon friction} \int d f_4 = -\varphi \int \frac{Q\, d_1}{2}\, d\,x = -\varphi \int P \frac{d_4}{2}\, \frac{l\,y}{x^2}\, d\,a.$$

With the help of the table containing the values of *a* cos *a*, sin *a*, *h*, *y*, and *x*, could easily be found, and with their help about 130 diagrams were constructed; the area of each one was measured, compared with the indicated work, and the result given in tables at end of paper and Diagrams I and II (Plate XII). As will be seen, Tables I.*a* and II.*a* are worked out for six different lengths of connecting rods, varying from ∝ to ¾ of piston stroke, and for ten different diagrams, the cut-off varying from $\frac{1}{10}$ to $\frac{10}{10}$, and also for a triangular diagram. Tables I.*b* and II.*b* will probably be found more handy than the others, marked I.*a* and II.*a*, as here the values are classed according to the mean pressures, and not according to the cut-off; in every other respect the two tables are identical. There is a decided difference in the crank-pin friction during the up and the down stroke, the loss during the down stroke (irrespective of piston weight) being greatest, but as there are only few engines which work with steam on one side of piston, the tables have been simplified by giving the mean values. The use to which they can be put will be plain from their appended explanation, but in case it should be thought necessary to find the friction for diagrams which are not contained there, the following examples may be of help:

In the diagram of an engine in which the connecting rod = 2 stroke (Fig. 3) and cutting off at $\frac{2}{10}$ stroke with a back pressure of .12 P, the mean pressure would be .40, because the area of A B C D E is .52 P., of E F *a* D it is .12 P., and therefore that of A B C *a* F is .40 P. The co-efficients are—

	Crank friction.	Guide friction.
For the first area	1.074	.187
For the second	1.018	.184
The co-efficient for the difference is	1.091	.185

because—

$$\frac{1.074 \times .52 - 1.018 \times .12}{.52 - .12} = \frac{.43632}{.40} = 1.091,$$

because—

$$\frac{.187 \times .52 - 1.94 \times .12}{.52 - .12} = \frac{.07396}{.04} = 185.$$

The friction of a diagram of the shape of Fig. 4 can be found in the

FIG. 3.

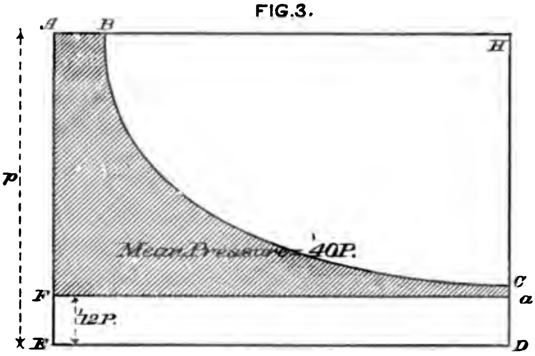
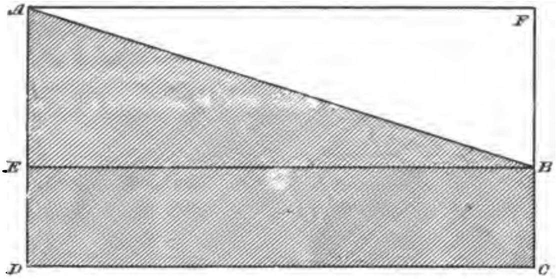


FIG. 4.



same way. Let the initial pressure be represented by 1 and the end pressure by .5, then the mean pressure will be .75; if the connecting-rod = 3 strokes, the co-efficients will be as follows:

	Crank friction.	Guide friction.
Triangular diagram	1.006	.194
Lower diagram	1.008	.194
Co-efficient of entire area	1.007	.194

Had the area of A B E been made equal to the area of B C D E, and the one subtracted from the other, very little would have been left; such a diagram would represent the action of the oscillating masses; this shows that they produce very little extra friction. When the connecting rod is shortened as much as $\frac{3}{4}$ the length of stroke, this friction will only amount to as much as would be due to a parallel diagram of which the height represents .023 of the greatest pressure exerted by such masses, so that they may safely be neglected. The same is nearly the case with the weights of piston, rods, and shafting; as long as the up and down pressure on the piston is greater than these combined weights they will have no effect, because they only increase the load during the down stroke, and diminish it during the up stroke; but as soon as it grows smaller than the weights, they alone produce the friction, and the steam pressure only alters the distribution during the up and down stroke; this will, perhaps, explain the reason why some engines, especially land engines, lose comparatively more when running light than when loaded. If, however, the engines work horizontally, the weight of part of the connecting and piston rod will either constantly increase or diminish the friction, which depends on whether the crosshead is working on its lower or upper surface. The crank-pin friction will also be increased by an amount which is probably a little less than that due to half the weight of the connecting rod, but can only be worked out for special cases. In double and treble engines the friction of the journals is slightly reduced, for it appears that in non-expansive engines with two cranks at right angles, the loads on them balance each other during two quarters of each revolution; in treble engines two cranks are always balanced, and it is therefore probable that in the former case a part (depending on the position of the journals) of one-half of the friction, and in the latter a part of two-thirds of the friction is saved. The following examples will still further explain the tables, while showing to what use they may be put. The dimensions of one of these engines are taken from the supplement to Burgh's "Modern Marine Engineering," those of the other from "Engineering," vol. xxi, p. 129, exemplifying two extreme cases of engine proportions. As regards the size of crank shaft in relation to the stroke, I have assumed the friction to be $\frac{1}{20}$ of the load, and have worked it out for two cut-offs in each case, with the following result:

	Tug *Figla*, Fig. 5 (Plate XIII).		*Bell Bird*, Fig. 6 (Plate XIII).	
Piston stroke	12 inches.		12 inches.	
Connecting-rod	24 inches.		28 inches.	
Diameter of crank-pins	7 inches.		2½ inches.	
$l : S$	2.		2⅓ inches.	
$\frac{d_2 + d_3}{2s}$	$\frac{1}{1.71}$		$\frac{1}{4.8}$	
Mean pressure	.847	.50	.847	.50
Guide friction, indicated horse-power	.0103	.0093	.0083	.0071
Crank friction, indicated horse-power	.0903	.0996	.0180	.0177
Total friction, indicated horse-power	.1006	.1089	.0263	.0254

In the case of the tug *Filga* the friction is about four times greater than in the other boat. You will see that the chief reason is the very short stroke and large crank pin; it therefore is probable that the friction might have been considerably reduced if the height of engine and length of the stroke had been increased a little, unless this would increase the gland and piston friction, so that possibly the total loss would have been greater instead of less; but putting that aside and assuming that there was no room for heightening the engine, then the stroke could, with slight advantage, have been lengthened at the expense of the connecting rod. The following numbers will show that the engine might have been improved, but only slightly:

Stroke	12 inches.	13 .5	14 .6	15 .5
Connecting rod	24 inches.	20 .25	17 .5	15 .5
Diameter of pin	7	7	7	7
$l : S$	2	1 .5	1 .2	1 .0
$\frac{d_2 + d_3}{s}$	1 : 1 .71	1 : 1 .93	1 : 2 .10	1 : 2 .20
Mean pressure	.50	.50	.50	.50
Total friction	.1089	.1023	.1011	.1013

So that as far as this friction is concerned, a stroke of 14.6 inches, with a connecting rod of 17.5 inches, will give the best results; but then the engines would not work so uniform as they now probably do, which will be seen later on. If, however, the above proportions are the most advantageous that can be adopted, such engines will never compare favorably with ordinary ones, especially as the chief friction is to be found in the bottom ends, where nobody wants it.

Returning now to the Diagrams I and II (Plate XII), it will be seen that the crank-pin friction is least for an engine cutting off at about half-stroke with an infinitely long connecting rod, and that even when it is about twice as long as the stroke, which is now the general practice, the friction is less than it would be with a full diagram and a long rod. So long as the cut-off is kept above $\frac{3}{10}$ and the connecting rods are longer than $\frac{6}{5}$ of stroke, there is little extra friction to contend with either in single, double, or treble engines; the guide friction will also be comparatively small if those limits are not overstepped, though the length of the connecting rods has here a far greater influence.

Before referring to the uniformity of motion of engines, I shall have to draw your attention to the oscillating masses, and their influence on it.

All oscillating masses store up energy at the commencement of the stroke and redeliver it at the end; if there are others which just then are absorbing energy it will not be visible in the diagrams of the turning movement of the crank, and for such cases only a co-efficient expressing the necessary inertia of a fly-wheel will be of value, while in other cases, where these masses can of course vary within wide ranges just as the stroke and the cut-off have been made to vary, the determination of such a co-efficient would be very complicated and would require enormous tables, and even then they would only be valuable in so far as they could show some general law. Retaining, then, all the previous notations, the downward piston speed would be—

$$\frac{du}{dt} = \frac{du \, . \, da}{da \, . \, dt} = r \sin a \, . \, \omega$$

the downward acceleration is—

$$\frac{d_2 u}{d t_2} = r \, \omega \, \frac{d \, (\sin a)}{d t} = r \, \omega^2 \cos a$$

and the force which the accelerated mass m of piston and rods exerts is—

$$-m\frac{d^2 u}{d t^2}$$

producing a negative turning moment

$$M = -m r^2 \frac{\omega^2}{2} \sin 2a.$$

In an engine with two cranks at right angles this value would be—

$$M = -\frac{r^2 \omega^2}{2} \sin 2a\,(m_1 - m_2).$$

If the moment of inertia I_1 of the connecting rod, taking its top end as fulcrum, be substituted for m_2, the turning moment produced in a single engine will be—

$$M_1 = -\frac{r^2\omega^2}{2} \sin 2a \left(m_1 - \frac{I_1}{l^2} \right).$$

Double engine $M_{11} = -\frac{r^2\omega^2}{2} \sin 2a \left(m_1 - m_2 - \frac{I}{l^2} + \frac{I_2}{l^2} \right).$

Treble engine
$$M_{111} = -\frac{r^2\omega^2}{2} \left[\sin 2a \left(m_1 - \frac{I_1}{l^2} - \frac{m_2 + m_3}{2} + \frac{I^2 + I_3}{2 l^2} \right) - \cos 2a \sqrt{\frac{3}{4}} \left(m_2 - m_3 + \frac{I_3 - I_2}{l^2} \right) \right]$$

If, however, the weights in the double and in the treble engines are equal, both M_{11} and M_{111} will equal nil, that is the oscillating masses may be neglected in such engines when inquiring into the uniformity of motion. However, when these weights exert no disturbing influence on the turning moments they act as fly-wheels would do, and as the effect is very simple, and as I have never seen them looked at in this light before, I hope a short development of the principle will not be out of place.

If you imagine a turning moment to be acting on a weightless shaft to which a fly-wheel having a moment of inertia T has been fixed, it will produce an angular acceleration.

$$\theta = \frac{M}{T}.$$

If the same moment is made to act on a weightless crank shaft to which a connecting rod and piston having the combined mass (m) has been connected, it will produce a downward acceleration—

$$\frac{M.}{m \,.\, r \,.\, \sin a}$$

and an angular one—

$$\theta_1 = \frac{M}{m \,.\, r^2 \sin a^2}.$$

If θ and θ_1 are to be equal, then T must equal $m\,r^2 \sin a^2$. By substituting the masses of piston and rods for the different engines, in equations showing the angular acceleration of a crank at any time or angle, the following equations will be the result: T_1 T_{11} and T_{111} are the inertia of imaginary fly-wheels—

Single engine $\quad T_{\mathrm{I}} = r^2 \left[\left(m - \frac{I}{l^2}\right) \sin a^2 + \frac{I}{l^2}\right]$

Double engine $T_{\mathrm{II}} = r^2 \left[\left(m_1 - \frac{I_1}{l^2}\right) \sin a^2 + \left(m_2 - \frac{I_2}{l_2}\right) \cos a^2 + \frac{I_1}{l^2} + \frac{I_2}{l^2}\right]$

Treble engines $T_{\mathrm{III}} = \left[\left(m_1 - \frac{I_1}{l^2}\right) \sin a^2 + \left(m^2 - \frac{I^2}{l^2} + m_3 - \frac{I_3}{l^2}\right)\frac{1}{2}\right.$
$\cos a^2 - \left(m^2 - \frac{I^2}{l_2} - m_3 + \frac{I_3}{l_2}\right)\sqrt{\frac{3}{4}} \cos a \sin a + \frac{1}{4}(m_2 + m_3) + \frac{I_1}{l^2} + \frac{3}{4}$
$\left.\frac{I_2 + I_3}{l_2}\right]$.

If $m_1 = m_2 = m_3$ as before, then—

$$T_{\mathrm{II}} = r^2 \left(m + \frac{I}{l^2}\right),$$
$$T_{\mathrm{III}} = r^2 \frac{3}{2}\left(m + \frac{I}{l^2}\right)$$

which can be expressed as follows: In double and treble engines the oscillating masses, if equal, act in the same way as a solid fly-wheel of an equal total weight, and having for diameter the length of the stroke. In single engines only a very small part of the total masses act as a fly-wheel. Therefore, the following tables, which have been compiled on the assumption that the oscillating masses produce no disturbing influences, will only be strictly applicable to the case of double and treble engines where the weights of the pistons and rods of the different engines are about equal, though they will no doubt be found of value in some of the other cases. The values were arrived at in the following manner: Let ψ represent the ratio of greatest increase or decrease of energy to the amount of work indicated per stroke, and η the co-efficient of uniformity, then

$$\eta = \frac{\psi \times \text{work per stroke}}{T \times \omega^2},$$

and the inertia of fly-wheel $T = \frac{\psi}{\eta \cdot n^3} \times 1{,}550{,}000 \times$ I. H. P.

where n = number of revolutions per minute, η and every other value except ψ is known; to find this, a great number of diagrams representing the turning moments of single, double, and treble engines, with six different grades of expansion and eleven different lengths of connecting rods, were drawn according to the following formula:

$$\frac{du}{dx} = h\left(1 + \frac{y}{x}\right),$$

$$M = P.\frac{du}{da} = P.\,h\left(1 + \frac{y}{x}\right).$$

Then the line of mean resistance was drawn through them, all the different areas above and below this line were measured, and the largest one divided by the area during one half revolution, which is the required coefficient ψ. To find Q, the weight of the fly-wheel, substitute the proper value of ψ out of the table into the following formula, where $R =$ radius of fly-wheel and η co-efficient of uniformity.

$$Q_1 \text{ tons} = \frac{\psi}{\eta\, n^3} \times \frac{I.H.P}{R^2} \times 22200.$$

The weight of a solid fly-wheel is

$$Q_2 \text{ tons} = \frac{\psi}{\eta\, n^3} \times \frac{I.H.P}{s^2} \times 177{,}500.$$

If, however, in a double or treble engine Q is equal to or less than the total weight of the pistons and rods, then no fly-wheel is necessary.

Looking at the Diagrams II, III, IV (Plates XII and XIII), of the value ψ, which give one an idea of the necessary weight of fly-wheel or revolving masses, it will be seen how very much smaller it is for the treble than for the double and single engines, and that it does not vary, as has been generally assumed, inversely as the grade of expansion, but that it increases at the same rate as the length of stroke while the connecting rod remains constant. In Diagram V (Plate XIII) you will notice that the values diminish between the lines, representing a cut-off of about $\frac{1}{10}$ to $\frac{3}{10}$ stroke. This depression puzzled me greatly at first, but I found on looking at the diagrams that for the above cut-offs the maximum turning moment of one engine is exerted just when one of the other cranks is either at the top or bottom of stroke; the result is a very uniform diagram. The same thing occurs in double engines for a cut-off of about $\frac{7}{10}$ stroke, which will explain the depression in Diagram IV (Plate XIII). Looking at Diagrams IV and V, it will be seen that the co-efficients of a double or treble engine, in which the ratio of $l : s$ is 1 : 1 and 2 : 1 respectively, are nearly identical for grades of expansion varying from $\frac{3}{10}$ to $\frac{6}{10}$, and that beyond these limits the treble engine has the advantage; it therefore appears strange that, except for centrifugal pumps, the three-cylinder engines which are now being constructed should have what appear to be long connecting-rods, especially as by lengthening the stroke at the expense of the diameter and other dimensions, the friction in the bottom end is reduced considerably.

In conclusion, I wish to say that as all the diagrams on which the calculations were based are theoretical ones, the results will never be absolutely in accordance with practice, but they give a very good idea of the influence of the two dimensions often referred to above, and will, I trust, facilitate the work of those who now and again have to go into this subject for new types of engines. Rounding off the corners of the diagrams will have the effect of leveling the lines, *i. e.*, increasing the small co-efficients and diminishing the large ones.

Summing up the results of all these calculations, you obtain a few rough formulæ.

Weight in tons of a solid fly-wheel the diameter of which equals the length of the stroke is Q.

$$\text{Single engines } Q^1 = \text{I. H. P.} \times \frac{177500}{\eta\, s^2\, n^3} \left(.30 + \frac{s}{6\,l} - \frac{e}{10}\right)$$

$$\text{Double engines } Q^2 = \text{I. H. P.} \times \frac{177500}{\eta\, s^2\, n^3} \left(.07 + \frac{s}{8\,l} - \frac{e}{20}\right)$$

$$\text{Treble engines } Q = \text{I. H. P.} \times \frac{177500}{\eta\, s^2\, n^3} \left(.055 + \frac{s}{20\,l} - \frac{e}{20}\right)$$

$$\frac{s^2\, Q \times 2240}{8 \times 32.2} = T.$$

The friction is approximately

$$\text{Guide friction} = Fg = \text{I.H.P.} \times \varphi + \frac{s}{l} \times 4.0$$

$$\text{Crank or journal friction} = Fc. = \text{I. H. P.} \times \varphi \times \frac{d}{s} \times 1.6$$

TABLE I *a.*—*Mean normal pressure on guide bar.*

Cut off / Stroke.	Connecting rod+Piston stroke.									$\frac{\text{Mean pressure.}}{\text{Initial pressure.}}$
	∝a+1	6+1	3+1	2+1	3+2	6+5	1+1	6+7	3+4	
.1	0	.0557	.112	.170	.231	.295	.370	.451	.553	.33026
.2	0	.0613	.123	.187	.254	.324	.406	.497	.601	.52189
.3	0	.0637	.128	.194	.264	.338	.423	.516	.621	.66119
.4	0	.0648	.130	.198	.269	.345	.431	.525	.633	.76652
.5	0	.0652	.131	.200	.271	.348	.434	.529	.636	.84657
.6	0	.0651	.131	.199	.271	.348	.434	.528	.637	.90649
.7	0	.0645	.130	.198	.268	.346	.431	.526	.633	.94967
.8	0	.0639	.129	.195	.265	.342	.426	.520	.628	.97851
.9	0	.0635	.128	.194	.264	.339	.421	.517	.623	.99482
1.0	0	.0634	.128	.194	.264	.338	.420	.516	.622	1.00000
Δ	0	.0634	.128	.194	.264	.338	.420	.516	.622	.50000

TABLE II *a.*—*Mean pressure on crank pin.*

.1	1.190	1.190	1.200	1.210	1.225	1.246	1.273	1.305	1.340	.33026
.2	1.060	1.060	1.067	1.074	1.088	1.107	1.136	1.165	1.203	.52189
.3	1.001	1.002	1.010	1.017	1.030	1.050	1.080	1.113	1.153	.66119
.4	.977	.978	.984	.993	1.006	1.025	1.058	1.094	1.137	.76652
.5	.969	.970	.978	.985	1.000	1.020	1.053	1.089	1.136	.84657
.6	.975	.976	.983	.990	1.004	1.026	1.058	1.092	1.138	.90649
.7	.983	.985	.989	1.000	1.013	1.033	1.064	1.098	1.144	.94967
.8	.992	.994	.988	1.009	1.020	1.040	1.070	1.104	1.148	.97851
.9	.998	1.000	1.005	1.015	1.026	1.046	1.073	1.109	1.151	.99482
1.0	1.000	1.002	1.008	1.018	1.030	1.050	1.075	1.110	1.152	1.00000
Δ	1.000	1.001	1,006	1.018	1.036	1.060	1.091	1.128	1.173	.50000

TABLE I *b.*—*Mean normal pressure on guide bar.*

$\frac{\text{Mean pressure.}}{\text{Initial pressure.}}$										
.1	0	.048	.093	.141	.190	.240	.299	.367	.448	.1
.2	0	.052	.104	.156	.209	.270	.335	.409	.500	2
.3	0	.056	.111	.167	.227	.290	.364	.443	.543	.3
.4	0	.058	.116	.178	.240	.308	.387	.470	.575	.4
.5	0	.060	.121	.186	.253	.321	.404	.492	.598	.5
.6	0	.063	.127	.191	.260	.333	.418	.508	.614	.6
.7	0	.064	.130	.196	.267	.342	.428	.520	.628	.7
.8	0	.065	.131	.199	.270	.348	.433	.527	.635	.8
.9	0	.065	.131	.198	.270	.348	.430	.528	.637	.9
1.0	0	.063	.128	.194	.264	.338	.420	.516	.622	1.0
Δ	0	.063	.128	.194	.264	.348	.420	.516	.622	.5

TABLE II *b.*—*Mean pressure on crank pin.*

.3	1.218	1.218	1.227	1.237	1.255	1.278	1.306	1.334	1.372	.3
.4	1.135	1.135	1.143	1.153	1.168	1.188	1.215	1.246	1.280	.4
.5	1.073	1.073	1.078	1.087	1.100	1.120	1.149	1.178	1.213	.5
.6	1.024	1.025	1.032	1.040	1.052	1.070	1.100	1.131	1.170	.6
.7	.991	.992	.999	1.008	1.020	1.038	1.069	1.104	1.146	.7
.8	.971	.972	.980	.988	1.002	1.020	1.054	1.090	1.136	.8
.9	.975	.975	.982	.989	1.003	1.025	2.058	1.091	1.138	.9
1.0	1.000	1.002	1.008	1.018	1.030	1.050	1.075	1.110	1.152	1.0
Δ	1.000	1.001	1.006	1.018	1.036	1.060	1.091	1.128	1.173	.5

EXPLANATION OF TABLES AND DIAGRAMS I AND II (PLATE XII).

To find the loss due to guide friction multiply the values in Tables I *a* or I *b* by ϕ × I. H. P.

To find the loss due to crank-pin friction multiply the values in Tables II *a* or II *b* by $\phi \frac{d}{s}$ × I. H. P. × 1.57.

ϕ=Co-efficient of friction.

d= Diameter of crank pin or shaft.

s=Length of stroke.

I. H. P.= Indicated horse-power.

The lines marked Δ give the values for triangular diagrams.

Ratio of maximum increase of energy to indicated work per stroke.

TABLE III.—SINGLE ENGINE.

Cut off / Stroke.	Connecting rod + Stroke.									Cut off / Stroke.
	∞+1	6+1	3+1	2+1	3+2	6+5	1+1	6+7	3+4	
.1	.329	.352	.374	.396	.417	.441	.465	.489	.515	.1
.2	.298	.324	.347	.372	.397	.423	.448	.477	.505	.2
.3	.277	.301	.326	.352	.380	.407	.435	.465	.496	.3
.4	.260	.283	.308	.336	.364	.393	.422	.454	.488	.4
.5	.248	.271	.295	.323	.350	.379	.410	.444	.480	.5
.6	.238	.259	.284	.311	.339	.367	.399	.434	.472	.6
.7	.228	.249	.273	.301	.329	.357	.389	.425	.463	.7
.8	.219	.241	.264	.292	.320	.349	.382	.417	.455	.8
.9	.212	.233	.257	.284	.313	.341	.373	.409	.448	.9
1.0	.205	.227	.250	.277	.306	.334	.367	.403	.440	1.0

TABLE IV.—DOUBLE ENGINE.

.1	.072	.092	.114	.136	.158	.179	.202	.227	.246	.1
.2	.049	.070	.092	.115	.139	.161	.186	.211	.236	.2
.3	.032	.052	.075	.099	.122	.147	.173	.199	.227	.3
.4	.020	.041	.063	.089	.113	.138	.165	.191	.222	.4
.5	.013	.033	.056	.082	.107	.131	.159	.186	.216	.5
.6	.009	.030	.053	.078	.104	.128	.156	.183	.212	.6
.7	.000	.030	.052	.078	.102	.126	.154	.181	.209	.7
.8	.011	.031	.054	.079	.103	.127	.152	.180	.206	.8
.9	.015	.036	.058	.082	.105	.128	.153	.179	.205	.9
1.0	.022	.043	.064	.086	.107	.131	.154	.179	.204	1.0

TABLE V.—TREBLE ENGINE.

.1	.0227	.0279	.0358	.0437	.0528	.0628	.0722	.0842	.0993	.1
.2	.0083	.0166	.0266	.0349	.0476	.0588	.0711	.0850	.1015	.2
.3	.0052	.0161	.0253	.0367	.0478	.0600	.0720	.0850	.1010	.3
.4	.0083	.0175	.0266	.0371	.0480	.0589	.0698	.0816	.0965	.4
.5	.0085	.0166	.0251	.0349	.0454	.0554	.0663	.0772	.0900	.5
.6	.0076	.0153	.0227	.0315	.0406	.0506	.0610	.0716	.0839	.6
.7	.0063	.0131	.0205	.0279	.0367	.0462	.0556	.0668	.0785	.7
.8	.0059	.0112	.0175	.0249	.0332	.0417	.0512	.0615	.0738	.8
.9	.0065	.0112	.0170	.0236	.0306	.0389	.0480	.0585	.0702	.9
1.0	.0059	.0109	.0163	.0223	.0292	.0373	.0462	.0567	.0680	1.0

EXPLANATION OF TABLES AND DIAGRAMS III, IV, V (PLATE XIII).

To find the inertia in foot-pounds of the revolving masses of an engine necessary to hinder the speed from varying beyond certain limits of the average speed, multiply the value in the tables by $\frac{\text{I. H. P.} \times 15{,}500{,}000}{\eta\, n^3}$.

To find the weight of a fly-wheel in tons which will produce the same effect, multiply the values in the tables by $\frac{\text{I. H. P.} \times 21{,}600}{\eta\, n^3\, R^2}$.

η = Co-efficient of uniformity $= \frac{\text{maximum speed} - \text{minimum speed}}{\text{mean speed}}$.

n = Number of revolutions per minute. R = Radius of fly-wheel. I. H. P. = Indicated horse-power.

DISCUSSION.

Mr. **Kirk.** My lord and gentlemen, as no one else seems inclined to rise and say anything after this elaborate paper, which has taken a vast deal of trouble to write, and a vast deal of trouble to think and calculate, I venture to stand up to suggest that such a paper as this should be held as read, because I venture to say although there are a good many here whose mathematical knowledge is much more up to the present day than mine, I do not suppose any of the gentlemen present would have followed this paper on its being read to them for the first time. I would venture seriously to suggest that although such papers are of infinite value in our transactions for private study, they are very much better taken as read. Mr. Stromeyer, I consider, is one of our very rising young men. He has produced—although this is not strictly relevant to the question—a very elegant instrument for showing the internal strains upon structures when they are not measurable otherwise. I hope this paper will be an encouragement, and I say it should be an encouragement to Mr. Stromeyer to pursue further his investigations upon such mechanical subjects as have come under his notice.

Mr. **Ravenhill.** I think all gentlemen present will agree with me that the engineering department at Lloyd's has done good service to this Institution during our present session. Yesterday we had a long and a very elaborate discussion on Mr. Parker's paper, and to-night we have two other papers from that department—one, my lord, which you characterized just now as being beyond discussion. Mr. Kirk has suggested that such papers should be taken as read. I venture to differ with him, because I think in an institution of this kind it might be well if some of us could now and again be benefited by hearing these nice mathematical questions freely and fully discussed; but to do so it would be necessary for such papers to be in the hands of our secretary some few days before the meeting of the Institution, in order that they might be sent out to the members, who would then have time to go through them quietly, and join in a full discussion, a discussion which would be well worthy of this Institution, and conduce to the knowledge of us all. I have omitted to say one thing: I think our thanks are due to the writer of this paper specially, for it must have cost him enormous labor to go through all these calculations.

Mr. **Martell.** Those last remarks, my lord, really fulfill the object of my rising. You were pleased this morning to pay a compliment to the younger members of this Institution who took the trouble to prepare papers as well as to enter into a discussion of papers that are read here, and I should be very sorry if a young member like Mr. Stromeyer should imagine for a moment that he was "damned by faint praise" in any way. I hope that the rule which was, I think, very wisely framed by the council some year or two ago—that members wishing to supply papers, particularly scientific papers of this character, that require investigation before you can enter into them and discuss them—will be complied with in the future, and that the writers will endeavor to prepare them and send them in, so that they can be printed and sent out to members in time to enable them to read them, in order that the great trouble they have taken in preparing such papers may be more fully appreciated by the members and shown by their entering into discussions upon them.

Mr. MACFARLANE GRAY. Having twenty years ago written a series of papers on the friction of load in the steam-engine, I may perhaps be better able than most of you to judge of this paper. I must say that I am very highly pleased with it; it carries the investigation on to the higher parts of the subject, and to a question which I only approximated—the effect of the cut-off. I know very well the great amount of labor that there is in preparing such a paper. When some years ago Mr. Stromeyer, then a sea-going engineer, had occasion to come before me for Board of Trade certificates, I formed a very high opinion of him, and, like Mr. Kirk, I think him one of the rising engineers. I have therefore had great pleasure, while listening to this paper, in observing the glow of enthusiasm which animated Mr. Stromeyer while dealing with this subject. Much of the paper is perhaps above the merely practical requirements of the engineer, but even engineers do not live by bread alone, and we have something higher here. Mr. Stromeyer's countenance has, it has seemed to me, been beaming out to us the truth of the beautiful old statement, about "the excellency of knowledge," that it "giveth life to them that have it." Some of the young men present, who may not be able to follow the mathematical investigations, may yet carry away this impression from Mr. Stromeyer, and if they follow that leading, that I consider will be a most valuable result. In Mr. Stromeyer we have a fine illustration of the value of the continental system of technical education, to which so much attention is now being directed in this country.

Mr. STROMEYER. I would wish to remark that I was afraid if this paper had not been studied beforehand it would not be so thoroughly understood as I would wish it to be. I therefore sent it in a considerable time beforehand, but I think there has been some delay, and I was not able to correct it till very lately. That was through my being abroad, or else the paper would have been sent away before that time. I do not know that I have anything else to remark, except that I am very thankful for the reception the paper has met with.

The PRESIDENT. Gentlemen, I am sure I may convey to Mr. Stromeyer your thanks for his paper. We must take into consideration the enormous amount of labor, time, patience, and knowledge which he has brought to bear upon its preparation. The encomiums that have been passed upon him have been perhaps fewer than many of us could have wished, but they have been of a very high order. I think the suggestion that has been made by more than one member is well worth consideration—that any papers of this elaborate and difficult description—

for to me I honestly confess they are purely Hebrew, or any other unknown tongue—should be laid before the members in time for them to thoroughly digest them and prepare themselves for the discussion of such elaborate questions as are raised in papers of this description. I convey to Mr. Stromeyer, I am quite sure with your concurrence, our very heartiest thanks.

NOTES ON SCREW PROPULSION.

By Charles Hall, Esq., *Member.*

[Read at the twenty-second session of the Institution of Naval Architects, April 8, 1881; the Right Hon. the Earl of Ravensworth, president, in the chair.]

There is perhaps no subject connected with naval architecture and marine engineering which combines the qualities of apparent simplicity and real difficulty in a more marked degree than the study of the screw propeller.

It is not surprising that to the tyro this has been a fascinating subject for invention; at first sight the thing seems so delightfully self-contained, so free from puzzling details and working parts, that it appears to be within the comprehension of any one; and consequently persons of all ages and classes have striven after some particular alteration of shape or curvature of surface which they imagined would solve all difficulties once for all, like squaring the circle or discovering the philosopher's stone.

Even among those whose business it is to design screw propellers there is still much of the same tendency; indeed there are very few of even the best-informed who will venture to say that any clear or satisfactory comprehension of the subject has yet been arrived at. It is possible, of course, from the results of experience and observation to form a good general idea of the screw propeller which will suit any given type of ship, but the writer knows of no one who has yet succeeded in reducing these practical observations to formulæ upon which much reliance can be placed, and it is probable that there is no subject upon which the opinions and practice of leading steamship builders in this country differ in a greater degree than on that of propeller designs.

A good illustration of this fact may be found in proposals which have come under the notice of the writer during the last few months in connection with designs for propellers for three ships of similar size and power, and intended for employment on the same trade. It would be obviously undesirable to name the three builders, and they are therefore placed alphabetically.

	Diameter.	Pitch.	Blade area.
A proposed	18 feet 6 inches	23 to 34 feet (increasing)	102 square feet.
B proposed	19 feet	28 feet 6 inches	92 square feet.
C proposed	20 feet 6 inches	27 feet 6 inches	110 square feet.

All three are eminent firms of great experience and ability, yet it is manifestly impossible that all can be right, and in the present state of knowledge on the subject who can with absolute certainty say wherein either is wrong?

Although the object aimed at by experimenters has, in the great majority of cases, been to improve the screw propeller by such alterations of configuration as will lessen the supposed effects of centrifugal action or other fancied sources of loss, it will scarcely be disputed by any practical man that the true secret of success lies in the due proportion of the three cardinal elements—diameter, pitch, and blade surface—to each other, in such manner as to give the best possible balance to the engine power; and that the shape of blades (within certain limits), their thickness, and the condition of their surfaces, are matters of minor importance.

We thus see cases in which most unlikely-looking propellers, ugly, coarse, and heavy, give good results, whereas others of far more pleasing finish and design are failures; and, again, it may frequently happen that a screw which has been unsuccessful on one ship may give excellent results on another of similar size, but of different type. The same observation holds good also when the employment of the ship is varied, the screws suitable for a ship employed on the Atlantic trade being wasteful when she is employed in the more generally favorable weather of other lines, and at lower rates of speed.

The writer has had the privilege during the last twenty-three years of enjoying exceptional opportunities for studying this question, and it has long been his endeavor to reduce the mass of observations which have come under his notice to some shape on which to found definite rules. It has, however, been his experience that, notwithstanding the great number of experiments which have been made, very few have much real value, on account principally of want of system in their arrangement, and because in no case have they been sufficiently exhaustive.

This may be attributed principally to the disfavor which has until lately attached to model experiments, which afforded no reliable information until the late Mr. Froude threw light on their true relation to the results of the ships which they represented. In consequence of this disfavor experiments have been principally made on the ships themselves, or on steam yachts; and as frequent changes of propeller are costly, and a large amount of time and trouble is absorbed in the necessary comparative trials, it has been a very exceptional thing for more than a dozen, or at most a score, of experiments to be made with any one vessel. The investigations have also in most cases been confined to alterations of pitch and diameter, leaving the equally important point of blade-surface very much in the background.

It occurred to the writer some twelve months ago, that, since the difficulties which have so long surrounded model experiments have now been in a certain degree removed by the researches of the late Mr. Froude, it may be possible to obtain a great deal of useful information by making systematic trials with models of various types of ships, so as to tabulate the results of a very large number of screw propellers of various proportions.

By the kindness of Messrs. Denny & Co., of Dumbarton, and Messrs. Caird & Co., of Greenock, he was provided with ¼-inch scale models of several steamers built and building by those firms; he fitted these models with very powerful clockwork, and during the summer of last year a series of about 80 trials was made with a view of ascertaining the results of model screws representing a diameter of 17 feet 4 inches and 28 feet (uniform) pitch, but with varying amounts of blade area.

The model principally used had the following proportion to the actual ship:

	Ship.	Model.
Length	380 feet	7 feet 11 inches.
Beam	38 feet	9½ inches.
Draught	17 feet 2 inches	4. 29 inches.
Mid section	541 square feet	33. 81 square inches.
Displacement	4, 011 tons	81 pounds.
Revolutions of screw	58 per minute	565 per minute.
Speed per minute	1, 302. 5 feet	188 feet.
Speed per hour	12. 85 knots	1. 855 knots.
Power developed	2, 400 horse-power	813 foot-pounds.

The displacement and midship section of the model are as the cube and square of the scale respectively. The speeds are shown in accordance with Mr. Froude's law, viz, as the square roots of the lengths, or 6.9285 to 1; but it will be noticed that the power developed does not quite come to the same proportion as the displacement, being 97,417 to 1, instead of 110,592 to 1.

This discrepancy arises from the excess of slip on the screw of the model as compared with that of the actual ship, being nearly four times, so that more power is expended in proportion, and the model results resemble those of the ship when driving against a head wind.

The cause for this increase of slip is somewhat obscure, and the writer hopes to present some special observations upon it on a future occasion. It is perfectly certain that the difference does exist, and the writer's experience on this point is confirmed by Mr. Griffiths, who has noticed a similar increase of slip in his model experiments.

Without entering into tedious details of all the trials made, the result may be stated as follows:

Taking as a standard the power expended by the largest screw in one minute, the screw in question representing 17 feet 4 inches diameter, 28 feet pitch, 147 feet area, and the power delivered per minute 765 foot-pounds, it was found that the time occupied by the series of screws in delivering the same power was—

	Seconds.
Screw of 147 feet area	60
Screw of 137 feet area	59.2
Screw of 127 feet area	58
Screw of 117 feet area	56.5
Screw of 107 feet area	54.6
Screw of 97 feet area	52.3
Screw of 87 feet area	49.6

In every case the reduction of area was lateral, the pattern being cut down gradually from a full Griffiths type to a parallel blade, and fresh castings were made each time.

On reducing the above results to a curve, it was found that the time varied as the .42 power of the areas, or, roughly, as somewhat less than their square root.

Subsequent application of this rule to actual ships, in cases wherein the propellers have been altered, have fully borne it out, provided the reduction of area was made laterally, as in the model screws. One case may be mentioned, in which a large ship was fitted with a new screw having larger pitch and smaller area than the former one, the design being to so proportion the two elements that the same resistance should be presented by the new screw as by the old one.

The chief engineer writes that on putting the engines to their old working point, with the old pressure of steam, he obtained precisely the same engine speed to which he had long been accustomed under similar circumstances of draught, trim, &c., thus exactly comfirming the expectations formed when the screw was designed.

The writer is now engaged in repeating his experiments, for the purpose of verifying them, using a new model of different type; and he proposes to make all his future trials progressively, in order to obtain speed and power curves for comparison with those of the actual ship.

Further series of trials will also be made during the summer with propellers of other proportions of pitch and diameter, the area being in each case varied as before; and blades are being prepared for comparison of thick with thin sections, rough with smooth surfaces; the areas will also be varied by reduction at the point and root respectively, in addition to the lateral reductions already tried.

The writer ventures to hope that permission may be granted by the admiralty authorities for the trial of these models at the establishment used by the late Mr. Froude at Torquay, so as to determine their curves of resistance. If this can be done, a comparison of the actual curve of the ship with those of the model will be exceedingly valuable and instructive.

It occurred to the writer, during the progress of one of these trials, to make an experiment which may be of some interest. A piece of string was accidently caught in the screw and twined round the boss without making any sensible reduction in the speed; the boss was therefore enlarged by winding more string upon it, until one-fourth of the blade surface was masked, that is to say, the blades were practically shortened one-fourth, and on trying the model it was found that the speed fell off from 188 to 180 feet per minute only. The blades were then further masked, so as to cover one-third of their length, and the speed was brought down to 158 feet per minute, showing how small a proportion of the work is done at the roots of the blades.

It should be mentioned that the writer is indebted to Mr. Griffiths, one of the veterans of screw propulsion, for many useful hints as to the method of carrying out the experiments described. The writer has, however, endeavored to improve upon the system followed by Mr. Griffiths, by the use of better models and greatly increased propelling power, as well as more accurately made propellers. The writer has repeated the experiment which Mr. Griffiths described to this Institution on a recent occasion, and has found that the alteration in the position of the screw, advocated by Mr. Griffiths, effected with all varieties of screws a palpable improvement.

The maximum gain on the long fine models used was, however, only 7 to 8 per cent., whereas Mr. Griffiths, with a model of 6 beams, obtained 11 per cent.; and it should be noted that the gain is very trifling, unless the screw is moved back to a distance of about half its diameter abaft the usual position.

It was not the writer's intention to have presented a paper on this subject until his experiments had assumed a more complete form, but it was suggested to him a few days ago by Mr. Denny that the results already obtained might be found interesting, and the writer has therefore communicated them, in the hope that his efforts to obtain information in the way described may perhaps induce others to follow up the same line of investigation.

DISCUSSION.

Mr. **John**. My lord, I only wish to say a word or two on this paper. I think we should all be very pleased when a gentleman like Mr. Hall devotes a large portion of his time to experiments and gives us the results of them. It was the fashion not many years ago to scoff at model experiments, but I think Mr. Froude taught us that from models there was an enormous amount of valuable information to be obtained which could be made applicable to ships. Therefore I can only hope the meeting will thank Mr. Hall for the care he has evidently bestowed on these experiments, and I must express the hope that he will continue them, and favor us with the results.

Mr. **Froude**. The only remarks that I will make this evening on this subject are in reference to the value of experiments on screw propellers made by means of models. I should be very sorry to be understood to say that screw experiments with models were not of great value, but I think, at present at any rate, they are likely to be of less value, or at any rate less directly trustworthy, than experiments on resistance of models of ships. Experiments on models of screw propellers are not so certain to give immediately trustworthy results as experiments on models of ships. In the first place, the phenomenon is on an exceedingly small scale. The surface of the model of the propeller, the thickness of it, even if the model itself is of a considerable size, is extremely minute, and we cannot have the same confidence that the phenomenon is really the same, or is readily relatable to the full-sized phenomenon, as we can when we are calculating the resistance of a ship from the resistance of a model. The forces that we are dealing with are also exceedingly small. The basis of the calculation of the efficiency of the propeller is the rotating force, and that in a model is an exceedingly small thing—it is a small thing in itself compared to the resistance, and it has to be applied by a mechanism which is, in my experience, impossible to get as delicate as the mechanism necessary simply to transmit a direct propelling force, such as the strain that is necessary to tow a model.

Captain LINDEN. My lord, I wish to communicate a fact which is perhaps not known to those present here. It is that an officer connected with the Russian Navy has proposed quite a new propeller with flat surfaces, without any curved surfaces at all. When he started his idea he was disbelieved, and it was said that his ships would not move at all, but still he prevailed on the authorities to be allowed to try it and carry out the experiment. He was allowed to fit his propeller to one of the torpedo boats, and the trials were conducted with great severity, and the speed obtained was the same as with the former propeller, and there was even a gain of half a knot. It is questionable perhaps whether the gain must be attributed to the peculiar form of the propeller or to some other causes, but still the speed was maintained when the propeller with flat surfaces was fitted to a Whitehead torpedo. I thought the communication of this fact might be of interest to this Institution.

Vice-Admiral SELWYN. In some recent experiments which I have witnessed and made I found that a perfectly smooth surface, exactly like the pulley of a block, carries around a quantity of water which is not at all inferior to the same circumference if it be supplied with fans or blades. This is a propeller introduced by Mr. George Wilson. But more than this, I find that this instrument, perfectly smooth in its character, can be converted into a dynamometer. In ten minutes with 310 revolutions a propeller of 14 inches in diameter at the bottom of the groove and 18 inches at the top can heat the water contained in its casing from 98 to 108 degrees of temperature, showing that it does lay hold of the water in a most remarkable manner. The application of this to screw propellers and its result is this, the screw propeller at ordinary speed carries with it a skin of water similar to that which accompanies the ship, and if the speed be excessive for the particular propeller, that is converted into a solid disk of water of a width due to the width of the blades. If the disk is of a rather larger diameter than the diameter of the screw propeller itself, under those circumstances the screw propeller will revolve, taking very little power from the engines, and without giving any push whatever to the ship. No such conditions would be found in ordinary practice, but the fact may lead us to consider the value of the surface friction or adhesion much more closely than we have hitherto done, because it is capable of developing a power to which none of us have given much heed as yet.

Mr. HALL. My lord and gentlemen, I hope that as we go on to further experiments on this subject we may be able to arrive at a closer knowledge of it. Of course, my appliances for the purpose are not so elaborate as those of the Torquay establishment, but I have the great advantage of being able to watch closely the results of the actual ships, as well as those of their models, and even with the simple apparatus I have used I think it will be possible to arrive at results which will have a very definite value. I hope soon to convince Mr. Froude that this is the case. The difficulties are of course great, but I do not think they are insurmountable. It appears to me of pressing importance that we should endeavor to solve this problem, or at least put our state of knowledge regarding screw propulsion on a more creditable basis, and it seems to me the duty of all who have the necessary opportunities to make such efforts as they can, in however humble a way, to obtain more light on so important a question ; the enigma will surely yield to diligent, careful, and systematic investigation. After twelve months' work with my models, I find that the results are a good deal influenced by the expertness gained in their management, and I shall be glad to assist any one who may be disposed to take up a similar line of experiment, by showing them how they can acquire that expertness more easily and at an earlier period than might otherwise be possible.

The PRESIDENT. I think it only remains for me to convey our thanks to the author of this paper on behalf of the Institution.

Plate [redacted] XII.

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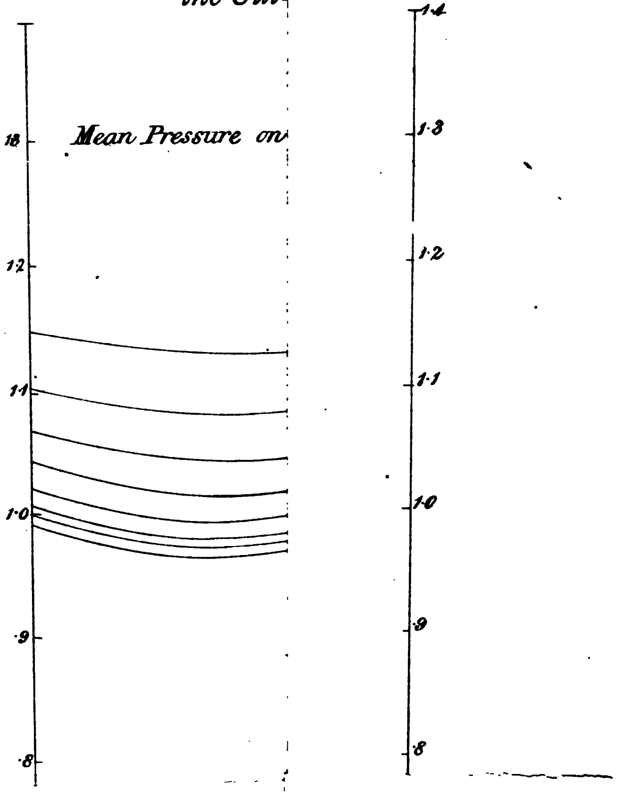

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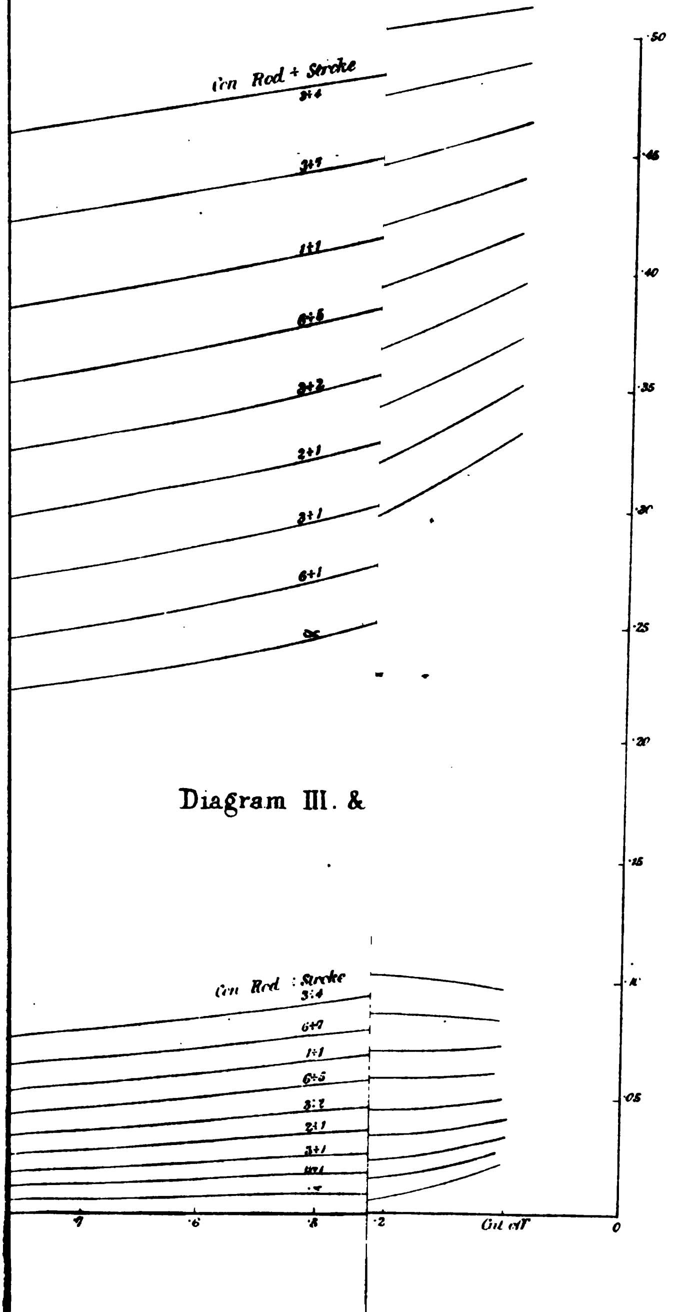

Diagram III. &

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